

PSYCHOLOGICAL REVIEW PUBLICATIONS

# Psychological Review

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## THE PSYCHOLOGICAL REVIEW

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### THE SO-CALLED 'GENERAL CHARACTER TEST'

BY PAUL A. WITTY AND HARVEY C. LEHMAN

*The University of Kansas*

Numerous attempts have been made recently to measure 'general character.' General character is sometimes assumed to be innate, unitary, and amenable to measurement and quantitative description. Although occasionally regarded in this manner, it is considered more often to be the sum total of rather closely related specific traits. The problem of measurement of general character is, if this latter concept be adhered to, similar to that encountered in measuring general intelligence.<sup>1</sup>

Recent periodical literature gives evidence of a widespread demand for and belief in the measurement of general character or its components. In March, 1925, May and Hartshorne published an annotated bibliography of 68 tests dealing with the measurement of character (1). They excluded purposely from their list rating scales and tests only indirectly related to the measurement of moral character. In July, 1926, they published a more comprehensive list containing 196 tests dealing with the measurement of character and personality traits (2).

The tests of moral character devised thus far are of the following types: (1) Those involving reactions to hypothetical questions regarding moral situations; (2) those which involve reactions to laboratory or classroom situations; and (3) those which involve reactions to actual life situations.

<sup>1</sup>This concept of intelligence and its measurement is reported by S. S. Colvin in the Twenty-First Yearbook of the National Society for the Study of Education 1924, Part I, pp. 11 ff.

Tests of the first type usually involve ethical discrimination under purely hypothetical conditions. To assume that with such a test one is actually measuring character seems unwarranted, for the precise relationship between *knowing* and *doing* has not yet been determined. It is obvious that in many instances mere knowledge of right and wrong is *per se* little safeguard against wrong-doing. Tanner and Barnes found that students will condemn acts which they take no steps to prevent (3). Also, practical observation gives evidence that most persons engage in certain activities which they readily condemn as unethical.

When performance tests are utilized in the laboratory or in the classroom, it is very questionable whether the responses obtained are symptomatic of the reactions that will follow apparently similar situations met in life. What appears to be a similar situation in life is often a slightly changed one and the response therefore modified.

A practical difficulty encountered in trying to test character traits under laboratory conditions is the fact that serious temptations are seldom placed before the human subject. Much of the conduct disapproved by society is so tabooed that experimenters are hesitant to utilize the actual items in the laboratory. Therefore, tests which attempt to employ life situations in measuring moral stamina are very imperfect.

He who would test morality meets another insuperable barrier in attempting to formulate a valid test. No experimenter has as yet assembled a battery of tests which includes anything approaching a random sampling of moral issues. This difficulty is encountered because of the *multitudinous* ways in which a trait manifests itself, and the consequent difficulty in defining moral, non-moral, and immoral issues or acts. 'Trustworthiness' will serve as an example. One is willing to trust his money, but not his wife, to a certain individual. To another he is content to trust an idea or a secret but not his money.

Another difficulty encountered in measuring character in the laboratory is the fact that a human being is a constantly

changing organism. He reacts very differently from time to time depending upon his physiological condition and his past and immediate background of experience. 'Honesty' will exemplify this point. An individual may be extremely honest in financial matters if he be affluent and comfortable. But, if he be hungry or cold or suffering from many of the desires that accompany poverty, honesty is quite a different matter. The business man may practice honesty in his dealings in order that he may maintain or extend his credit. The down-and-outer may be less concerned about his credit and more concerned about his empty stomach. It is clear that the temptation to be dishonest has widely different degrees of strength depending upon the condition of the subject. Also, there are numerous ways in which such a trait as honesty may assert itself. To be honest financially is decidedly different from being honest intellectually. A test of moral stamina or of a single character trait, after being *clearly defined and delimited*, must take into account: (1) The strength of the stimulus, and (2) the condition of the subject, including the strength of his resisting power.

The strength of a temptation (the situation) is probably itself a function of several variables such as, (1) the individual's particular likes and dislikes; (2) his felt need at the moment of temptation, *i.e.*, whether he be famished or in a state of satiety, etc.; (3) the supposed liability of being detected; and (4) the general social atmosphere or the environmental conditions that surround him.

The strength of the resisting power of the subject is also a variable depending upon factors such as, (1) the subject's physiological and mental state at the moment of temptation, and (2) his need at the time of the temptation's occurrence, etc.

#### CHARACTER NEITHER A UNITARY TRAIT NOR A COMBINATION OF UNITARY TRAITS

The difficulties cited above which attend character measurement are due probably in part to the failure of the investigators to realize that the traits they are attempting to measure are not unitary entities.



Symonds (1924) vigorously criticized the contributions to date pertaining to character measurement because of the carelessness with which terminology is employed (4).

. . . are we trying to measure something that actually exists? When I read over a list of traits such as intelligence, neatness, humor, beauty, refinement, sociability, likeableness, snobbishness, conceit, vulgarity, I am wondering if there is any one thing that corresponds to these names. It smacks very much of 'faculties'. 'Whatever we may name exists,' is a tacit assumption that we too easily make. If we have found that memory and imagination are particular, depending on the material with which they deal, so much the more snobbishness, vulgarity, or even honesty and trustworthiness. . . . So before we try to measure trustworthiness, incorrigibility, self-assurance, or what not, perhaps it would be worth while to question the existence of these things as universal individual qualities that work quite regardless of the special situation. Low test intercorrelations may be due to just this—that slight change of the situation will lead to a totally different reaction (5).

Other investigators have arrived at conclusions similar to those of Symonds. Brown employed 26 tests to measure 'suggestibility' and concluded:

It must be said at once that there are no individual differences which are sufficiently conspicuous to justify the experimenter in calling one person 'very suggestible' and another 'not suggestible.' There are no individuals who have consistently high or consistently low indices of suggestibility through a series of tests. On the contrary, the experimenter is struck by the fact that the most skeptical individual will yield at times with surprising readiness to the suggestion, while a person who has yielded to some tests with very little apparent resistance will unexpectedly become very recalcitrant (6).

Trow attempted to measure confidence in one's own judgment by having the subjects judge such subjective things as logical fallacies, ethical situations, etc. He secured 26 sets of ratings each involving 20 judgments. He concluded that there seems to be no such thing as a general character trait of 'confidence' (7).

The following analysis suggests that thrift is probably not a unitary trait:

Thrift seems to be a bundle of more or less loosely connected special habits,—habits with regard to and conservation of materials, earning, saving, spending and repairing. So an index of a person's 'thriftiness' would be an inventory of his responses to all these various situations. . . . We need to come down out of the sky and think less in terms of kinds of personality, or of traits of character and more in terms of habits of conduct, or specific reactions to well defined situations (8).

Goodwin B. Watson attempts to explain a low correlation which he obtained between the odd and the even test items of Voelker's battery of tests of 'trustworthiness' as follows:

It does not detract from the value of the individual tests but only questions the probable unity of the trait called 'trustworthiness' that this writer finds a correlation of only  $+ .20$  between the score of boys on the 'odd' tests of Voelker's battery and the 'even' tests of the same battery. Voelker reports a correlation of from  $+ .21$  to  $+ .85$  between the first battery and the second which contains paired tests (9).

The quotations listed above suggest that numerous investigators may be attempting to measure entities that exist only in name. In the face of repeated protests a few experimenters still seem to be clinging tenaciously to the hypothesis that general character is a unitary trait, and that it may be measured by a simple device. The most recent attempt of this nature that has come to the attention of the present writers is that of Woodrow and Bemmels who conclude that "the overstatement test . . . constitutes a very good test of general character in pre-school children" (10). These writers conceive general character to be the average development attained in all desirable specific traits, weighted perhaps according to social standards of their importance. That these specific traits are measurable, the authors assume, and state that it would not take a large battery of tests as good as the overstatement test to yield a very satisfactory measure of general character in pre-school children. The validity of

the overstatement test was ascertained by correlating the results of this test with teacher judgments of general character development in the children. Two groups of children were used. The groups contained 17 five-year-old, and 14 four-year-old children, respectively. The correlation between the amalgamated rank in character and overstatement was  $+ .86$  for the five-year-old group, and  $+ .43$  for the four-year-old group. The authors conclude that the test employed therefore is a valid measure of general character.

One must infer from Woodrow and Bemmel's conclusion that they consider teacher estimates a reliable criterion of general character. And they must consider general character an innate capacity similar to the mental tester's conception of general intelligence since they assume character can be gauged at pre-school ages. The present writers are hesitant to accept such hypotheses. Abundant evidence can be adduced to show that morality and numerous so-called traits of character are acquired.

What is character? Is it an entity, having a static positive existence of its own which issues into conduct, or is it merely the sum total of our conduct tendencies? Of course the latter fits in better with modern psychological concepts. If we believe character to be largely native then we are justified in attempting to measure general qualities; if we believe the character is largely acquired we ought to examine critically 'qualities' or 'characteristics' and proceed more analytically. This latter procedure demands more attention (11).

General character appears to be largely the result of experience, not an innate trait. Until evidence is adduced to the contrary, it seems logical to accept this explanation. It therefore seems that attempts to measure general character are spurious when simple devices are applied which fail to take into account the varied experience of each child.

#### GENERAL CHARACTER TESTS INVOLVE QUESTIONABLE METHODS AND CONCLUSIONS

One of the most conspicuous examples of illogical reasoning and unwarranted assumption is evidenced in the recent

tendency to attribute all desirable traits to gifted<sup>2</sup> children on the ground that the results of the so-called moral tests are actually measures of these desirable traits.

There is at hand sufficient experimental evidence to show that superior children mingle easily with other children, and as a rule show little difficulty in adjusting themselves either to an older or to a younger group (12).

We believe that the results of this division of our study, especially of the Raubenheimer-Cady-Wyman tests, have an important bearing on the educational treatment of gifted children. They strongly suggest that *the danger of over-intellectualization at the expense of normal development along social and moral lines is probably much less than some have believed it to be* (13).

The present writers have secured certain data regarding character development for a group of fifty gifted children. The interpretation of the data by them is decidedly different from the usual tendency in this regard. The group includes fifty children of I.Q. 140 or above (Stanford Revision of the Binet-Simon Test Rating). Forty-two of these children were identified in grades III to VII of the public schools of Kansas City, Missouri; eight were chosen from neighboring towns. Each gifted child was paired with a mentally average child (I.Q. 90-110) of like age, sex, and environment. Effort was made to secure average children in reference to mental age, educational age, and school progress. Comparable data were secured for both groups. The study was conducted in January, 1926.

#### OVERSTATEMENT TEST DATA

In studying these children, the writers employed several overstatement and honesty tests. The first test is similar to that devised by Raubenheimer and Ruch. Section A of the test consists of the titles of fifty books, twenty of which are 'faked.' The children were instructed to check titles of books which they had read. Any fictitious title checked was

<sup>2</sup> The term 'gifted,' as here employed, posits I.Q. of 140 or above (Stanford Revision of Binet-Simon Test Rating). No other implications are intended by the writers.

considered an overstatement. Table I displays a comparison of the test results for the gifted and for the control groups.

TABLE I

## OVERSTATEMENT TEST DATA

	Gifted Group	Control Group
Percentage of book titles overstated .....	4	18

Section *B* of the overstatement test is composed of eighty informational questions of the following form: Do you know who discovered America? The children were instructed to mark the questions in the following manner: If you know the answer very well, mark it '2,' if you know it fairly well, mark it '1,' if you know nothing about it, mark it '0.' Section *C* of the test consists of eighty statements that correspond with the questions asked in Section *B*. Each of these statements is accompanied by several answers, only one of which is correct. The children were asked to underline the correct answer. The amount of overstatement on Section *B* was determined by the number of '2's,' '1's,' and '0's,' that were answered right and wrong respectively in Section *C*. The results secured are presented in Table II.

TABLE II

## OVERSTATEMENT TEST DATA

	Gifted Group	Control Group
Percentage of statements marked '2,' <i>i.e.</i> , answer very well known, and answered correctly .....	85	58
Percentage of statements marked '2,' but answered incorrectly, <i>i.e.</i> , an overstatement .....	15	42
Percentage of statements marked '1,' <i>i.e.</i> , answer fairly well known, and answered correctly .....	61	36
Percentage of statements marked '1,' but answered incorrectly, <i>i.e.</i> , an overstatement .....	39	64

The superiority of the gifted in the overstatement tests is conspicuous. Is this test a valid measure of character development? Such data are taken often to reveal differences in general character. Voelker felt that the overstatement test was the best of his assembly for gauging trustworthiness (14). Cady considered it an important indicator



of corrigibility, and Raubenheimer thought that he found the test serviceable in differentiating 'potential delinquency' from 'reliability,' 'stability,' and 'healthy-mindedness' (15). Woodrow and Bemmels throw all caution to the winds and declare the overstatement test to be a very good measure of 'general character' (16). The present writers feel that data from overstatement tests reveal little regarding the general character of the subjects since the tests appear simply to evoke *habitual classroom responses*. They are invalidated further by the fact that the strength of the stimulus and the resisting power of the individual are not equated for the children tested.

It is obvious that the situation confronting the child taking an overstatement test has different degrees of strength, depending upon whether the child has no, little, or much knowledge of the subject at hand. In the ordinary classroom situation the gifted child is sure that most of his responses will be correct. It is probable that if he were thwarted and 'unsatisfied' by numerous failures on difficult subject-matter, he might behave quite differently if a chance to overstate his knowledge occurred. So far as the present writers are aware no overstatement test was equated for the subjects' knowledge of subject-matter included in the tests permitting overstatement.

Some writers attempt to validate the overstatement test as a measure of character by correlating the results so obtained with teacher estimate of character development of the subjects studied. They assume that the teacher judgment is a valid representation of the child's development in this regard. Two 'competent judges'<sup>3</sup> (their teachers) were asked to give general character ratings to the fifty gifted children studied by the present writers. These ratings were correlated with the overstatement test scores. The  $r$ 's were +.57 and +.60 respectively. Are we to suppose therefore (like Woodrow and Bemmels) that the overstatement test is a valid measure of general character? Certainly not!

<sup>3</sup> The reader will recognize no doubt the term 'competent' used in the customary manner in discussion of teacher judges.

What do teacher ratings show? *Simply that high academic attainment influences favorably teacher ratings in every other regard.* The familiar halo effect of scholarship is evidenced herein clearly. The present writers were impressed with the tendency of the teachers of gifted children to exaggerate the presence of desirable traits and underestimate the presence of undesirable traits when judging these children. Table III affords clear-cut evidence of this trend.

TABLE III

## TEACHER JUDGMENTS OF GIFTED CHILDREN'S PLAY

Number of children judged . . . . .	48
Percentage judged to play an average amount . . . . .	47
Percentage judged to play more than an average amount . . . . .	45
Percentage judged to play very little . . . . .	7

According to the classroom teacher estimates, 92 per cent of the gifted children play an average amount or more with other children.<sup>4</sup> *According to the children's own testimony, they engaged in exactly the same number of activities as the control group.* It is of interest too that the gifted group was found to engage in *less* social activities than the control. Yet the teachers reported that the gifted child engaged in a *conspicuously* larger number of social plays and games than the control child. Obviously, the teacher judgments were influenced appreciably by scholarship. The optimism of the teacher in regard to the gifted child's response to discipline is revealed in Table IV. The teachers reported that 98 per cent of the gifted children respond well to discipline and that only 2 per cent do not do so.

TABLE IV

## TEACHER JUDGMENTS OF GIFTED CHILDREN'S RESPONSE TO DISCIPLINE

Respond well to discipline . . . . .	98
Do not respond well to discipline . . . . .	2

Observation of these children over a period of three years has convinced the writers that several are decidedly difficult

<sup>4</sup>The teachers were asked to estimate amount in terms of *number* of activities engaged in by the child.

to govern. Three of them are actual problem cases at the present time. They are, however, the 'teacher's pride and joy' in certain types of academic endeavor. These data show very clearly that character tests cannot be validated by employing teacher judgments as a single criterion. Indeed, the careful student may question whether teacher judgment has *any* practical value in validating character tests.

### HONESTY TESTS

An objective test of honesty displayed in adherence to directions yielded further indication of the superiority of the gifted children. In this test the children were asked to close their eyes and draw a line connecting five crosses arranged in a circular form three inches apart upon a page. Whether or not the child cheated by opening his eyes was determined by the degree of accuracy with which the task was performed. The superiority of the gifted children in this test is indicated in Table V.

TABLE V

HONESTY TEST DATA

	Gifted Group	Control Group
Percentage who cheated by opening eyes . . . . .	10	48
Percentage who did not cheat by opening eyes . . . . .	90	50

Do these data reveal a *genuine* superiority in the character of the gifted? By no means! This honesty test shows simply that *in the classroom under academic influence* (a specific situation) the gifted child displays greater fidelity to directions than does the average child. The superiority of the gifted in this regard may be accounted for in the following manner. The gifted child is accustomed to secure success in generous amounts by adhering to directions but the average child is not so invariably successful. The average child therefore feels the need for varying his conduct to secure success and approval. He resorts to numerous defenses to compensate for lack of knowledge. He overstates his knowledge and alters directions in order *to secure success*. This test therefore reveals little regarding honesty in general

because it fails to take into account the *condition* of the child at the time of the examination. Nor does this test take into account the multitudinous ways in which honesty may be displayed or the effect of slight modifications of the situation upon the response.

#### THE NATURE OF MORALITY

If morality cannot be considered a unitary trait or a composite of specific traits, what is this elusive quality? To the present writers morality posits the sum total of specific habit reactions which facilitate adjustment in society and which society approves. Morality may be considered as a process of habit acquisition, never as a specific entity or static quality. Man's nature is moral or the opposite to the extent that he has chosen habits of action which enable him to function in society for the good of himself and of others. There is an abundance of evidence to support this concept of morality. This concept squares with behavioristic psychology. It accords too with present-day practices in abnormal psychology. The successful teacher of feeble-minded children attempts to *develop* morality by developing specific desirable habits.

Very different is this position from that which holds that every feeble-minded child is a potential criminal. Some have thought that a very large proportion of the feeble-minded is constitutionally immoral. Indeed an English statute has sanctioned this position, defining moral imbeciles as "persons who from early age display some permanent mental defect coupled with strong nervous or criminal propensities on which punishment has little or no deterrent effect" (17).

This concept of moral imbecility suggests that morality is a unit characteristic, its absence resulting in the state so described. Of this Wallin says:

It may be doubted whether there are any real moral imbeciles. In my block of 2,774 consecutive cases only one was diagnosed as a 'moral imbecile' . . . but his conduct so improved in the course of a few years as to render the diagnosis questionable (18).

Porteus too arrives at a similar conclusion:



. . . we would remark that we have never seen a moral imbecile, meaning by that a person whose only distinguishable defect is a lack of moral sense (19).

Mateer, from working with psychopathic children refuses to accept the idea that character and morality are unitary traits.

Again, the acceptance of a study of intelligence function seems to save us from the need for attempting so-called character and personality analyses. It takes care of these. What is character? It is the sum total of an individual's ideas, memories, and imagings, especially as they relate to his assimilation of the behavior code of the group with which he lives . . . plus the estimation of the reliability of his acting in accord with this knowledge. This latter factor is a composite of his functional stability and his past experiences, that is, habits of action. Frankly, there seems to be nothing more to it. At a certain moment a certain decision must be made. The past experiences of the individual arouses certain tendencies toward certain types of behavior. Every factor in the environment which is old and familiar reënforces the tendency toward earlier forms of action or reaction. Associatively, the whole system is influenced in its pleasure-pain relationship to the contemplated act. . . . And the result? Action or behavior predetermined by that group of reaction habits which is the stronger (20).

If morality then is acquired and acquired in terms of specific habit-formation, our task in the schools is that of teaching children to choose intelligently those habits which will function for the good of others. This means that, instead of attempting to develop a few specific traits with the expectation that these traits will transfer to situations outside the classroom, we should seek out diligently those specific habits which we call good in life and make provision for their acquisition.

If one accepts the point of view that character is acquired in terms of *habits* of action (and who has any evidence to the contrary?) the attempts at character measurement appear spurious and unnecessary.

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## A MENTAL UNIT OF MEASUREMENT<sup>1</sup>

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The measurement scale of primary interest in psychophysics is the *S*-scale which may be called the psychological continuum in contrast with the physical or stimulus-continuum, the *R*-scale. In the original formulation of Fechner's law the *S*-scale was described as sensation intensity but this interpretation of the psychological continuum is now pretty much out of date. We now speak of the *S*-scale as though it measured so-called sense-distances in relation to the corresponding differences or distances on the scale of stimulus magnitude. It is my present purpose to offer a revision of the sense-distance interpretation of the *S*-scale so that it will be independent of the j.n.d. and the difference limen and to propose a mental unit for mental measurement instead of the ambiguous physical unit, the j.n.d. I shall also try to show that this revised interpretation of the *S*-value makes it a *continuous* function of the stimulus magnitude because Fechner's paradox does not exist.

This continuum which is designated *S* in Fechner's law,  $S = k \log R$ , represents, first of all, a *gradation of qualitative processes*. These processes may be described as subjective or physiological to suit individual and temperamental preferences. That distinction does not affect the psychophysical problem. The continuum implies qualitative variation and it may or may not also possess quantitative aspects. It may be unidimensional or multidimensional.

A point on this continuum may be designated only by a corresponding stimulus. The stimulus may in turn be designated (1) by a quantitative stimulus attribute such as intensity or magnitude, or (2) by a qualitatively varying stim-

<sup>1</sup> This is one of a series of articles by members of the staff of the Behavior Research Fund, Illinois Institute for Juvenile Research, Chicago. Series B, No. 109.

ulus attribute such as excellence of handwriting or the beauty of rug patterns or the degree of pacifism expressed in statements about peace and war. Any perceptual quality which may be allocated to a point on the psychological continuum is not itself a magnitude. It is not divisible into parts. It is not a sum of any mental or physical units. It is not twice, three times, or four times as strong, high, beautiful, or good as some other process on the same continuum. It is not a number. It is not a quantity. These are statements about the psychological continuum on which at least fair agreement among psychologists is to be expected.

But furthermore, the j.n.d. is not a reliable unit of measurement along the S-scale. Any point on the scale represents a unified indivisible experience. Any two such points represent two such experiences which may be qualitatively entirely different and in every way incommensurate, so that they may represent by their own immediate attributes perhaps no similarity by which any 'distance' between them could be measured.

With these negations granted, just how do these qualitative entities or processes become a measurable continuum? *They acquire conceptual linearity and measurability in the probability with which each of them may be expected to associate with any prescribed stimulus.* This is the crucial characteristic of the psychological continuum in terms of which psychological measurement is possible. The S-continuum is constructed or defined in such a manner that the frequency distribution of the S-experiences for any given stimulus  $R$  is normal. Each modal  $S_k$  experience is that particular S-experience which is most frequently associated with the stimulus  $R_k$ . Hence if measurement begin with the stimulus  $R_a$  for which  $S_a$  is the most common or modal S-experience, then some other S-experience,  $S_b$ , will be spaced far away, along the imaginary continuum, if the probability is low that  $S_b$  will be experienced with the stimulus  $R_a$ . It will be spaced close to  $S_a$  if the probability is relatively high that  $S_b$  will be experienced with  $R_a$ . Fortunately it is possible to verify experimentally the validity of this definition or construction of the S-scale be-

cause the continuum may be constructed separately for each stimulus and the attainment of internal consistency of the scale will indicate whether the correct form of frequency distribution has been used. Therefore the normal frequency distribution is not blindly assumed. It is tested for. If found incorrect, other forms of distribution may be tried in a similar manner. There is one assumption underlying this construction of the psychological continuum, namely, that all the stimuli in a series project the same *form* of frequency distribution on the S-scale, but it is not assumed that their dispersions are equal.

Since the dispersions which the several stimuli project on the S-continuum are not assumed to be equal, the natural unit for psychological measurement becomes the dispersion of one of these stimuli *measured on the psychological scale*. This is what I have called the discriminial dispersion or, more specifically, the discriminial error of each stimulus and it should not be confused with the customary error of observation. There are two fundamental differences, namely, (1) the discriminial dispersion or error is measured on the S-scale whereas errors of observation are naturally measured on the R-scale, and (2) the discriminial dispersion is the dispersion projected on the S-scale by a *single stimulus* whereas an observational error is naturally the pooled effect of *two stimuli*. The two stimuli are the two terms in the psychophysical judgment by the method of constant stimuli or they are the presented and the reproduced stimuli in the method of reproduction.

It should be noted that an observational error can itself be objectively produced and it can be directly measured on the R-scale. The discriminial dispersion, though more elemental in character than the observational error, cannot by itself be objectively produced. It can be measured only indirectly since it concerns the S-scale. Every judgment when objectively produced constitutes a single observational error which is loaded with *at least two* discriminial dispersions or errors. A single discriminial dispersion or discriminial deviation cannot by itself ever become an objective record and consequently its measurement must necessarily be indirect.



I have attempted to state briefly a definition of the psychological continuum and to show that a truly psychological unit of measurement may be established for the psychological scale. The psychological S-scale becomes then in effect a frequency scale as far as its experimental identification is concerned. It is an imaginary scale on which we allocate and space out the psychological counterparts of the several stimuli in the stimulus series. Since the stimulus series is regarded as strictly continuous, we define the corresponding psychological scale similarly so that any stimulus magnitude or quality may be allocated to a point on the continuous S-scale. Similarly, since the psychological continuum is not directly or physically accessible or controllable, we identify any point on the S-scale by a stimulus magnitude.

The customary definition of the S-scale as the measurement of so-called sense distance is not here entirely rejected but it is made more definite. As long as we define the S-scale as the measurement of sense distance and still deny that it measures sensation intensity or any other quantitative characteristic of sensation, there remains an unsatisfactory vagueness about the nature of the psychological continuum. If we insist that the sense quality is not itself an intensity or magnitude of any sort, how does it happen that we get quantitative measurement in the form of measured distances between these sense qualities which are themselves denied measurable and quantitative attributes? *What constitutes the sense distance that is measured between two qualitative entities?* That is a question concerning the very nature of the psychological continuum which, as far as I am aware, has not hitherto been answered. It is the answer to just this question that I have attempted in formulating a revised definition of the S-scale so that it may also fit the experimental facts.

Let the S-scale consist in a gradation of qualities by means of which we perceive any specified stimulus continuum. Allow that a given stimulus is not always perceived by the same process on the S-continuum. Let the quality most commonly perceived in the given stimulus be designated the



modal quality or process for that stimulus. Then we can assign numerical values to other qualities in the *S*-scale in accordance with the *frequency* with which they are perceived in the given stimulus. It does not matter whether the *S*-qualities or processes are in any real sense actually spaced out in a continuum so long as they behave as though they were so spaced out. Mental measurement depends according to the present interpretation on the frequency with which each of the processes constitutes the response to a given stimulus. It is reasonable to assume that two perceptual sense qualities or processes which are close together on the psychological continuum are qualitatively similar and that therefore either one of them may more or less readily be perceived in the same stimulus. *To the extent that two perceptual processes are qualitatively similar, to that extent will their probabilities of association with the same stimulus be nearly the same, and to that extent will they tend to be adjacently spaced on the imaginary psychological continuum.* It is sufficient for the purposes of mental measurement that the qualitative perceptual processes behave as though their respective probabilities of association with a given stimulus were a normal frequency distribution. The natural psychological unit of measurement becomes, then, the standard deviation of the frequency distribution for a specified stimulus. This unit of mental measurement I have called the standard discriminial error for the specified stimulus and it is of course measured directly on the psychological continuum. It is entirely independent of stimulus measurement. It is independent of the validity of Fechner's law. It is also independent of the validity of Weber's law. It is a valid unit of measurement even when the objective stimulus cannot itself be quantitatively measured.

It should be noted that the unit of mental measurement that I have proposed is not in any sense a j.n.d. The just noticeable difference is in every case a stimulus measurement. It is measured on the *R*-scale. Hence it is in reality a *physical unit* which in some situations can serve indirectly the purposes of mental measurement. The discriminial error is a *mental unit* of measurement since it is defined on the psychological

continuum. Its physical equivalent will vary from one situation to another depending primarily on the validity of Fechner's law or some other  $S$ - $R$ , relation for the particular perceptual function under consideration. *This proposed mental unit for mental measurement may be defined as the standard deviation of the frequency distribution projected by a standard stimulus on the psychological continuum.* I propose to call this mental unit of measurement *the standard discriminational error*. The assumption, the correctness of which will determine the validity of this unit of measurement, is that an  $S$ -scale with internal consistency will be obtained by spacing the perceptual qualities on it so that their probabilities of association with any given stimulus will be Gaussian. *With the psychological continuum so defined the standard discriminational error, as a mental unit of measurement, will be the standard deviation of the perceptual qualities perceived in a standard stimulus.*

#### THE CONTINUITY OF THE $S$ -SCALE

We have so far taken for granted the continuity of the  $S$ -scale but the descriptions of the psychophysical  $S$ - $R$  relation so frequently give the idea of a discrete series of steps or jerks that the error of such a notion needs clearly to be corrected. The typical description of the psychophysical  $S$ - $R$  relation starts with stimuli  $R_1, R_2, R_3$ , so nearly similar that they differ successively by one 'j.n.d.' Corresponding to these stimulus magnitudes are postulated  $S$ -values  $S_1, S_2, S_3$ . Then the implication is that any stimulus difference less than  $(R_1 - R_2)$  cannot be discriminated at all and that as soon as the stimulus difference becomes as large as  $(R_1 - R_2)$ , then suddenly the difference jumps into perceptibility and it is designated 'a least perceptible difference,' a sense minimum, the unit of mental measurement, the j.n.d. Even so clear a writer as Keyser<sup>2</sup> describes the  $S$ - $R$  relation essentially in this way as an example of the data on which we build our conceptual continuities. Even Titchener does not seem to avoid this possible misinterpretation. The following quotations from

<sup>2</sup> Keyser, C. J., 'Mathematical Philosophy,' 1922, Chapter XVIII.

Titchener illustrate the experimental discreteness which is apparently read into the *S*-continuum.<sup>3</sup> "Suppose that a brightness or a noise is given, and that we seek to determine the just noticeably brighter brightness, or the just noticeably louder noise. The experiment is identical with one of our 'friction' experiments; and its result is the ascertainment of a just noticeable sense-distance. Let us perform it at various points of the sense scale . . . . Now these are all least distances, minima of sensible distance." And similarly on p. xxxvii we read: "The facts that a stimulus must attain a certain magnitude in order to arouse a sensation at all, and that it must attain a certain magnitude in order to effect a noticeable change in sensation, are facts of the same order."

Another description of the *S*-*R* relation which is just as misleading as the quotations from Titchener begins with what is known as Fechner's paradox. This description runs usually as follows: let  $R_1$ ,  $R_2$ ,  $R_3$ , be three stimuli of decreasing order of magnitude. Let the difference ( $R_1 - R_2$ ) be so slight that it cannot be noticed, *i.e.*, less than the difference limen. Let the difference ( $R_2 - R_3$ ) also be less than the difference limen. Then it might still happen that the difference ( $R_1 - R_3$ ) would be a little larger than the difference limen so that it would be noticed. Then, so the argument goes, it would be possible to have the following psychological equalities:

$$\begin{aligned} R_1 &= R_2, \\ R_2 &= R_3, \end{aligned}$$

which would be inconsistent with the fact that

$$R_1 \neq R_3.$$

This is known as Fechner's paradox. The truth is that no one has yet found a just noticeable difference. The least sense distance has never been experimentally demonstrated, and Fechner's paradox does not exist.

Now, as a matter of fact, every one who works at all seriously in psychophysics knows that just noticeable differ-

<sup>3</sup> Titchener, E. B., 'Experimental Psychology,' Student's Quant. Manual, p. xxxiv.

ences have never been found, that it is necessary to specify quite arbitrarily a stipulated frequency of discrimination in order to put any sense in the j.n.d., that the phi-gamma hypothesis, or its variants, assume continuity of the psychometric function, and that experiments such as those of Brown<sup>4</sup> on very small stimulus differences indicate experimentally that sense minima in any genuine sense have not been found. It would seem best therefore to avoid these entirely misleading descriptions of the functional relation between the stimulus magnitude  $R$  and the psychological value  $S$ . These ambiguities of description disappear entirely if description of the  $S$ - $R$  relation be kept true to the experimental facts so that the psychological magnitude, or sense distance, between two stimuli be expressed *in terms of the observed frequency, or the probability, of its discrimination*. As the stimulus difference increases by gradations ever so slight, at least as far as they are experimentally controllable, the probability becomes higher and higher that the stimulus difference will be discriminated. In this sense, then, we may arbitrarily define the difference limen or the j.n.d. as that stimulus difference which has a probability of .75 of being correctly discriminated. This is the manner in which we actually do determine the limen and it is consequently inaccurate to allow the description of the  $S$ - $R$  relation to remain as though it consisted in a series of jerks with a constant friction load for every jerk. The description of Fechner's paradox may still stand as a curious and intriguing distortion of truth. The argument that the two perceptual qualities are psychologically equal because they are equal in a single perceptual judgment simply ignores the fact that the quality perceived in the stimulus is fluctuating or varying. True, they are equal in the judgment in which they are perceived to be equal, but the  $S$ -value of a stimulus is the mean perceptual value of many repeated perceptions of the same stimulus. My point here concerning the continuity of the  $S$ -scale is nothing new. It is merely a plea for consistency so that throughout the description of psychophysical

<sup>4</sup>Brown, W., 'The Judgment of Difference, etc.,' Univ. Calif. Publ. in Psychol., 1910, Vol. I., No. 1.

effects the continuity of the *S*-scale may be ensured since it is everywhere experimentally indicated.

This consistency is readily obtained if we will recognize that there is a difference between the *S*-value perceived in a stimulus on any particular occasion and the mean of the *S*-values perceived in the stimulus on many occasions. It is in the dispersion which a constant stimulus projects on the *S*-scale that we have the opportunity to establish a truly mental unit of measurement. It might seem that the average *S*-separation projected by a *pair* of stimuli would be a better unit of measurement; but such a unit would suffer from the logical disadvantage that the mental unit, so defined, would be loaded with two discriminial dispersions which would in any event have to be solved for. Since the dispersion projected by a single stimulus is more elemental than the pooled effect of two stimuli, it seems more natural to adopt the projected dispersion of a standard stimulus as the basis for a unit of measurement for the psychological continuum. This unit I propose to call the standard discriminial error. Its numerical treatment consists merely in assigning the value of unity to one of the two discriminial errors,  $\sigma_1$  and  $\sigma_2$ , in the law of comparative judgment.



### THREE PSYCHOPHYSICAL LAWS<sup>1</sup>

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In a previous article<sup>2</sup> I have shown that Weber's law and Fechner's law are separate laws so that it is logically possible to have a set of data in which either one of these laws is verified and the other one not verified. This distinction comes about primarily by what I have called the discriminial dispersion of the stimulus. It is a vital factor in verifying Weber's law but it is not logically a necessary factor in the verification of Fechner's law. If the discriminial dispersions of all the stimuli are equal, then the two laws can be verified by the same set of data although even then they constitute statements about different aspects of the data.<sup>3</sup> In the same article I have developed the logic by which a psychological scale or continuum becomes a possibility. In that connection I formulated a third psychophysical law which I called the law of comparative judgment. It is my present purpose to explain the differences and the similarities among these three psychophysical laws.

Before making comparisons it may be well briefly to review the statement of each of these three laws.

Weber's law is sometimes carelessly stated as follows: The just noticeable increase in a stimulus is a constant fraction of the stimulus. When the ambiguity of the term 'just noticeable' is removed, the law takes the following form: The stimulus increase which is correctly discriminated in any specified proportion of the attempts (except 0 and 100 per

<sup>1</sup> This is one of a series of articles by members of the staff of the Behavior Research Fund, Illinois Institute for Juvenile Research, Chicago. Series B, No. 108.

<sup>2</sup> 'Psychophysical Analysis,' *Amer. J. Psychol.*, 1927, 38, 368-389.

<sup>3</sup> In the previous article cited I made the statement (p. 386) that the two laws become identical when the discriminial dispersions are equal. That was not quite accurate. The two laws are then verified by the same set of data but they describe different aspects of the data.

cent) is a constant fraction of the stimulus magnitude. Since the experimental verification of the law practically always refers to the comparison of two stimuli as to relative magnitude, it is more accurate and satisfactory to state the law so that it really refers to this comparative situation involving differentiation of *two* compared stimuli rather than the perception of an increase in a *single* stimulus magnitude. Weber's law would then take the following form: Within the same modality and with constant experimental conditions, the ratio between two stimulus magnitudes which are correctly discriminated in any specified proportion of the attempts, excepting zero and 100 per cent, is a constant. Restating the law in more convenient form we have

$$\frac{R_2}{R_1} = K \quad \text{or} \quad R_2 = KR_1 \quad (1)$$

in which  $K$  is a constant and  $R_1$  and  $R_2$  are defined by the relation

$$P_{R_1 > R_2} = C. \quad (2)$$

From (1) we have

$$P_{R_1 > KR_1} = C,$$

in which  $C$  is any arbitrarily assigned constant between 0 and unity. This may be generalized into a complete algebraic statement of Weber's law, as follows:

$$P_{R > KR} = C \quad (\text{Weber's Law}). \quad (3)$$

Here the notation  $R$  refers to any stimulus magnitude,  $P$  is a proportion,  $P_{R > KR}$  is therefore the proportion of judgments ' $R$  is greater than  $KR$ ,' while  $K$  and  $C$  are constants. The constant  $C$  may be arbitrarily assigned. It is customary to specify .75 as the arbitrary value for  $C$ , while the constancy of  $K$  remains to be experimentally verified.

Another frequent but erroneous statement of the law is as follows: Sensations increase in arithmetical progression as the stimuli increase in geometrical progression. This is actually a statement of Fechner's law and not of Weber's law. It will be noticed that Weber's law says absolutely nothing about sensation intensities. These do not occur in the above

statement of the law and it is brought into the discussion of Weber's law only when this law is confused with that of Fechner. None of the variables in equation (3) above say anything directly about sensation intensities. The principal variables are the stimulus magnitude and the factor  $K$ , the constancy of which is subject to experimental verification.

Fechner's law is usually and correctly stated in the form

$$S = K \log R \quad (\text{Fechner's law}), \quad (4)$$

in which  $R$  again refers to the stimulus magnitude and  $S$  refers to the so-called sensation intensity. The factor  $K$  is a constant. The universality as well as the psychological acceptability of this law will probably be greatly increased if instead of interpreting  $S$  to mean the intensity of a sensation, we interpret it to mean a linear measurement along an abstract psychological continuum from the sensory process of a specified stimulus magnitude as an origin. The term  $S$  then refers to a measurement along a psychological continuum from an arbitrary but specified origin. These measurements can of course be objectively identified experimentally only in terms of their respective stimuli. But in either the literal interpretation of the term  $S$ , as an actually measured intensity of sensation or the more liberal interpretation of it as a measurement along an abstract psychological continuum, we have two variables,  $S$  and  $R$  which are related by a simple logarithmic function.

Notice that Fechner's law says absolutely nothing about any proportion of correct discriminatory judgments which constitutes an integral part of Weber's law. In fact, Fechner's law is not at all concerned with the phenomenon of confusion of stimulus magnitudes, a phenomenon the description of which Weber's law is essentially aimed at. It is possible to test Fechner's law by using exclusively supraliminal stimulus differences, whereas the testing of Weber's law requires obviously the use of liminal and infraliminal stimulus differences because Weber's law is primarily concerned with the phenomenon of confusion of stimuli. Fechner's law is a statement concerning the rather common functional relation between

the stimulus magnitude and the corresponding psychological continuum.

The law of comparative judgment <sup>4</sup> can be stated without approximations in the following form:

$$S_1 - S_2 = x_{12} \sqrt{\sigma_1^2 + \sigma_2^2 - 2r\sigma_1\sigma_2} \quad (5)$$

(Law of comparative judgment),

in which the two terms  $S_1$  and  $S_2$  define a linear distance on the psychological continuum,  $x_{12}$  is the sigma value of the observed proportion,  $p_{R_1 > R_2}$ , of judgments ' $R_1$  is greater than  $R_2$ ,'  $\sigma_1$  and  $\sigma_2$  refer to the relative ambiguities or discriminial dispersions of the two stimuli while  $r$  is the coefficient of correlation for the two discriminial deviations involved in each of the comparative judgments.

Note that this law is entirely independent of the stimulus magnitudes. Hence it is directly applicable for measuring the psychological continuum corresponding to any qualitative series, to any psychological values which are perceived as a continuum, even though the objective counterparts of these values may not be themselves measurable. Since this law deals with the discriminatory process independently of the objective stimulus magnitudes, there can be no objective criterion for the 'correctness' of each discriminatory judgment. The proportions which enter into the law of comparative judgment are not proportions of 'correct' judgments. They are merely proportions of *similar* judgments, preferably when each judgment is a choice between only two stimuli, omitting the alternatives of 'equal' and 'doubtful,' and when the procedure follows otherwise the method of constant stimuli. It does not depend on the phi-gamma hypothesis, because the stimulus magnitudes do not enter into this law.

In order to summarize the similarities and the differences between these three psychophysical laws, the following tabular statement may be useful. In the first column are listed the variables that are involved in these laws. The plus sign

<sup>4</sup> For a more complete statement of this law with different degrees of approximation for its experimental verification, see my article, 'A Law of Comparative Judgment,' *PSYCHOL. REV.*, 1927, 34, 273-286.

refers to the fact that the law in question does include this variable, while the minus sign indicates that the law does not say anything regarding it. The variables are as follows:

$S$  = a linear measurement on the abstract psychological continuum.

$R$  = stimulus magnitude

$p$  = proportion of judgments ' $R_1$  greater than  $R_2$ .'

$\Delta R = (R_1 - R_2)$  corresponding to the proportion of judgments,  $p_{R_1 > R_2}$ .

$\sigma$  = discriminial dispersion.

Variables	Weber	Fechner	Comparative Judgment
$S$ .....	-	+	+
$R$ .....	+	+	-
$p$ .....	+	-	+
$\Delta R$ .....	+	-	-
$\sigma$ .....	-	-	+

The first row of comparison shows that measurement along a truly psychological continuum is not involved in Weber's law but that it is involved in Fechner's law and in the law of comparative judgment. This may be seen by direct reference to the explicit statements of the three laws. The second row of comparison shows that the stimulus magnitude is involved in both Weber's law and Fechner's law but that stimulus measurement is not involved in the law of comparative judgment. The psychological continuum is defined according to this law in terms of the proportion of similar discriminatory judgments, and the validity of the continuum can be established only in terms of experimental consistency of all the observed proportions.

With the factor  $P$  goes also the phenomenon of confusion of stimuli, which is not involved in Fechner's law while it is involved in the other two. Still another way of describing the same difference is to point out that Fechner's law might be experimentally verified by a *single* set of observations of the stimuli selected so that they appear equally distant from each other. When the physical stimulus magnitudes are plotted



against a scale of equal appearing intervals, the logarithmic law should be at least roughly verified. Such a demonstration is not possible for either of the other two laws. For them it is necessary to have a rather long series of separate judgments with a *proportion* of judgments for each pair of stimuli. This is but another way of showing that Fechner's law is, strictly speaking, not explicitly concerned with errors of observation, whereas the other two laws deal *primarily* with the magnitudes of errors of judgment.

The stimulus increase ( $R_1 - R_2$ ) which yields a prescribed proportion of similar discriminatory judgments is involved in Weber's law but it is not involved in either of the other two laws. The stimulus increase is involved in the proportion which is a principal factor in Weber's law, but the proportion involved in the law of comparative judgment defines the psychological continuum without reference to the stimulus magnitudes. Hence the stimulus increase becomes a part of Weber's law but not of the law of comparative judgment.

The last row of the comparative table shows that the discriminational dispersion, the relative ambiguity with which each stimulus is perceived, is a part of the law of comparative judgment, but that it is not explicit in either Weber's law or in Fechner's law. It is in the possibility of variation in the discriminational dispersions of the stimuli that the separation between Weber's law and Fechner's law appears. I have previously shown that when the discriminational dispersions can be assumed or shown to be constant for all the stimuli in an experiment, then the same set of data will be found to verify both Weber's and Fechner's laws, but that if the discriminational dispersions are not constant, then one of these laws may be verified when the other one is not.

In brief, Fechner's law deals with the *apparent* interval in relation to the stimulus interval, while Weber's law deals with the *frequency of correct discrimination* of an interval in relation to the stimulus interval. Since apparently equal intervals are not necessarily equally often discriminated, the two laws become logically separated.<sup>6</sup> Fechner's law con-

<sup>6</sup>Thurstone, L. L., 'Equally often Noticed Differences,' *J. Educ. Psychol.*, 1927, 18, 289-293.

cerns the nature of the  $S$ - $R$  function, while Weber's law concerns the frequency with which adjacent stimuli are confused. It would be logically possible for Fechner's law to be applicable to a set of data even if errors of observation or confusion should never occur, but in such a state of discriminatory perfection Weber's law could not exist.

#### CATTELL'S FORMULATION OF WEBER'S LAW

Cattell's formulation of Weber's law, or a substitute for it, makes it a relation between (1) the magnitude of the stimulus and (2) the magnitude of the standard observational error, *expressed also in terms of the stimulus scale*. His literal statement is as follows:<sup>6</sup> "The error of observation tends to increase as the square root of the magnitude, the increase being subject to variation . . . ." Whether the exact relation is that of the square root function is not for the moment our primary concern. We are noting here primarily the variables involved in these laws. Cattell's formulation may be restated explicitly as follows:

$$E_1 = M\sqrt{R_1}$$

in which  $E_1$  is the standard observational error for the stimulus magnitude  $R_1$ , while  $M$  is a constant. Restating it more generally without committing ourselves to the square root function, we have

$$E_1 = M \cdot f(R_1),$$

which brings out clearly the two variables,  $E_1$  and  $R_1$  in terms of which Cattell has cast Weber's law. This is consistent with our tabular analysis in that the law involves the stimulus magnitude  $R$  explicitly. It involves  $\Delta R$  which is synonymous with the observational error  $E$ , and it involves the proportion,  $p$ , in the definition of the observational error. The standard error,  $E$ , can be defined as follows:

$$E = \sqrt{\frac{(\Delta R)^2}{n}}$$

<sup>6</sup> Fullerton and Cattell, 'On the Perception of Small Differences,' University of Pennsylvania Philosophical Series, 1892, p. 25.

for the method of reproduction so that  $p$  is about  $2/3$ , or in some other equivalent manner. No matter how the observational error,  $E$ , may be defined, it automatically locks the proportion,  $p$ , of the reproduced stimuli which are counted within the range  $(R + \Delta R)$  or  $(R + E)$ . Conversely, a prescribed proportion,  $p$ , for the method of reproduction automatically determines the error of observation,  $E$ . According to Cattell, Weber's law does not involve the sensory or psychological continuum,  $S$ . Speaking of Weber's law he says: (p. 23) "All the experiments made by the first three methods which we have described seem to us to determine the error of observation under varying circumstances, and not to measure at all the quantity of sensation." Nor does Weber's law recognize the subjective standard error of observation,  $\sigma_1$ , of single stimuli.

If Fechner's law, or a substitute for it which involves the  $S$ - $R$  relation, can be assumed, it is possible to express Cattell's objective observational error,  $E$ , in terms of our discriminial dispersion,  $\sigma$ , for single stimuli. The objective observational error,  $E$ , is measured on the stimulus continuum, whereas the discriminial dispersion,  $\sigma$ , is measured on the psychological continuum.

In attempting to make an algebraic statement of Weber's law Cattell writes (p. 21)

$$N = C \cdot \frac{\Delta S}{S},$$

in which he defines  $S$  as his stimulus,  $\Delta S$  is the increase in the stimulus  $S$  which can be just noticed, while  $N$  is also defined as a least difference; with insistence that it be a physical quantity also. I fail to see any meaning in it since there are three physical quantities,  $N$ ,  $S$ , and  $\Delta S$  and it seems that  $N$  and  $\Delta S$  are the same thing. This equation is not a statement of Weber's law. His verbal formulation is clearer. The main point we get from Cattell in the present analysis is that Weber's law does not directly concern the measurement of sensation intensity or the psychological continuum.

These three psychophysical laws have been stated algebraically in equations (3) (4) and (5) respectively. The principal variables are (1) the stimulus magnitude, (2) the psychological *S*-value, and (3) the degree of confusion of stimuli. It has been shown that each of the three laws relates two of these variables and ignores the third.

## A BEHAVIORIST'S DEFINITION OF CONSCIOUSNESS<sup>1</sup>

BY EDWARD CHACE TOLMAN

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This paper should have been called "The frantic attempt of a behaviorist to define consciousness." In fact, the doctrine I shall present seems even to me quite unprovable and to you it will no doubt seem something far worse. And yet so great is my faith that behaviorism must ultimately triumph that I should rather present even the following quite doubtful hypothesis than hold my mouth and say nothing. If we behaviorists can not present good theories, we can at least present as many bad ones as possible in order that by their successive refutation we may be forced finally either into discovering the correct theory, or, if there be none, into abandoning our behavioristic adventure altogether.

Before, however, attempting my definition of consciousness, let me first briefly outline the nature of behavior as I see it. Every behavior-act, in so far as its continued going-off is contingent upon there proving to be such and such specific features in the environment, must be said in so far to postulate or cognize those features. For example, when a rat, after learning, is ready to enter only the white alley of a discrimination box, and not the black alley, the continuance of this 'white-entering' behavior must be said to express a cognitive postulation as to the difference between white and black. Further, the fact that the continuance of this tendency to enter the white and not the black is also contingent upon the further circumstance that there prove to be a difference between food and non-food on the two sides of the box must be said to express also a cognitive differentiation between food and non-food. And, thirdly, the continuance of this entering

<sup>1</sup> Read before the Western Psychological Association, Los Angeles, Calif., June 17, 1927.



of the white rather than the black must be said to express in addition a cognitive differentiation as to the relative sign-relationships of white and black—the one as indicative of the food and the other as indicative of the non-food. For, if any one of these three sets of environmental facts should suddenly change, this behavior-act would break down. Thus, if there ceased to be a difference between the white and the black, or between the food and the non-food, or between the respective sign-relations of the white and the black with regard to food and non-food, the consistent entering of the white and avoidance of the black would no longer continue. In short, the continued going-off of this white-entering behavior-act assumes, postulates, these three specific sets of environmental facts and relations.

But this which we have thus proved for the discrimination-box behavior holds, it would seem, in a similar manner for all behaviors. Every behavior-act, in going-off and being what it is, expresses, implies, certain specific characters in the environment. And this is so because the continuance of its going-off can be shown to be contingent upon there actually proving to be such characters in the environment. If these expected characters are not found, the act sooner or later ceases or modifies itself. Behavior is driven by organic needs, and in going-off it postulates that the environmental characters and relations are such that it will prove an appropriate behavior for satisfying those needs. The going-off of a particular act postulates a particular complementary character in the environment. And this is to be assumed as true for all behavior-acts whether new and just learned or old and well-established by custom. The only condition is that these acts show themselves ready for alteration, if things go wrong.

In spite, however, of this conclusion that practically all behaviors are thus cognitive or postulative, we must note the further fact that many of these cognizing and postulating behaviors are none the less quite obviously automatic and unconscious. For a behavior to be postulative and cognitive, it is not necessary that it also be conscious. A well-established and quite automatically functioning habit act cognizes and

postulates the environment, in our sense that its continuance is contingent upon the environment proving actually to be so and so. But such a habit-act may none the less be quite unconscious.

What, then, we must now ask, is the further occasion and cause of consciousness? Our answer will be that *wherever an organism at a given moment of stimulation shifts then and there from being ready to respond in some relatively less differentiated way to being ready to respond in some relatively more differentiated way, there is consciousness*. For example, let us assume that our rat has up to some given occasion been responding in undifferentiated fashion to the white and black alleys. He may, perhaps, have been treating them both as distinct from some third chromatically colored alley, but as between these two, the white and black themselves, his behavior has not distinguished. On this particular occasion, however, we assume that something internal happens, such that he then and there switches from not being ready to respond to them as differentiated to being ready to respond to them as differentiated. The moment of this switch is the moment of consciousness. The organism then and there becomes conscious of the difference between black and white. On all the previous occasions his behavior treated the black and white as alike. On the occasion when the switch occurs his behavior first starts to treat them as different. It is this change to the new differentiation which we define as consciousness. The behavior after such a switch may in time become just as automatic as the behavior before it. Acts which imply more cognitive differentiation may be just as automatic as ones which imply less cognitive differentiation. It is only the switch-over when it occurs in a given moment of stimulation that defines consciousness.

What, now, is the mechanism of such switch-overs? In order to answer, we shall have to consider a new principle. this new principle is that organisms, at least the higher ones, are to be assumed capable not only of actual behaviors but also of what may be called mere behavior-adjustments. The nature of these behavior-adjustments is to be assumed such

that they in some manner bring the animal into contact with the same stimulus-results with which he would be brought in contact, if he should actually behave. The results of any proposed act may thus by means of a mere feint or adjustment to that act be brought into the present and become a conditioner for or against the act. To make an adjustment to an act is to achieve a *representation* (based, of course, upon what has happened upon previous occasions when this act or similar ones have actually been performed) of the probable stimulus-results to be expected from the act.

This doctrine of an ability by virtue of mere behavior-adjustments to represent the probable results of acts may well strike you as a pretty mystical affair. It may sound to you unworthy of consideration by any hard-headed scientist, let alone a behaviorist. And yet, I would ask you, what is Watson's own doctrine of implicit or sub-vocal speech, in so far as it has any cogency, other than a specific account of just such behavior-adjustments? Watson, of course, does not call them behavior-adjustments, but his gestures and sub-vocal contractions seem to have been devised by him to serve the very function which we are ascribing to the behavior-adjustment. His doctrine has cogency only in so far as he implies that gestures and sub-vocal speech serve to bring (*i.e.*, represent) to an acting or listening organism the type of stimulus results to be expected from an actual overt behavior, if it were carried out. The baby, when ruminating on what it wants, says sub-vocally among other things the word 'doll.' But this saying of 'doll' serves to represent the type of stimuli to be expected if the baby were actually to go and get the doll. If these represented doll stimuli are satisfactory, the child performs the actual act of going and getting. If not satisfactory, he rehearses, sub-vocally perhaps, the names of other toys.

The above is, of course, not quite the way Watson's own argument reads. It is, however I believe, the way it should read, and it is only because of such an implied reading that his argument has such cogency as it seems to have. I present this account to you here, however, not to ask you to accept

the doctrine of sub-vocal speech and sub-gesture as such, but rather to woo your minds gently to my own more general notion of the behavior-adjustment. The behavior-adjustment, whatever its neurological or physiological character, is to be conceived functionally as a surrogate for actual behavior,—and a surrogate which somehow serves to bring into the present, that is to make then and there active upon the organism, the stimulus-results to be expected from the corresponding actual behavior.

Assuming for the purposes of argument that you accept this doctrine, the next step will be to declare that it is these behavior-adjustments which produce or *are* consciousness. When a rat on some given occasion switches over from a condition of non-readiness to discriminate white and black to one of readiness to discriminate them, and, as we have said, thereby becomes conscious of the difference between them, this switch-over and this consciousness are mediated, we shall now declare, by a behavior-adjustment. In this case we shall assume it is a behavior-adjustment to the act of running and looking rapidly from the one color to the other. The stimulus-results which would come from such an actual running or looking would presumably be a complex pattern containing both the run-from color and the run-to color. The rapid passage from the one to the other would, that is, presumably result in a sort of Gestalt (?) containing both the colors set off in juxtaposition one against the other. And the adjustment to such a running would be to bring this resultant Gestalt into the moment before actual behavior. Thus, it would be possible for the animal, when faced with either color alone, to respond nevertheless discriminatively to the difference between them. Another point, however, must now be noted, namely, that after this new differentiating behavior has once become established, consciousness and the behavior-adjustment can apparently drop out and yet the new discriminating behavior continue.

We must assume that the complex stimulus-pattern of white in juxtaposition to black, or vice versa, is still needed for the continuation of the discriminating behavior. But we



shall assume that eventually this complex pattern results automatically by pure associative extension from the white stimulus alone or the black stimulus alone. Simple redintegrative bonds must become established whereby the stimulus-results from running back and forth are now automatically fused into either the white or the black stimuli alone.

So much for the consciousness of white versus black. We saw, however, at the beginning of this paper that the total behavior of choosing one alley rather than the other involves not only this differentiation of white from black, but also a differentiation of food from non-food. And it involves likewise a differentiation of the specific sign-connection of white from that of black. We now assert that the initial appearance of these other two differentiations also involves consciousness. And they also are to be explained by the functioning of behavior-adjustments.

The switch-over at some single moment of stimulation from not being ready to differentiate between food and non-food to being ready to differentiate between them would be mediated by a behavior-adjustment for running rapidly from the one goal to the other. Such a behavior-adjustment would present the complex Gestalt-result of the two types of goal compared one against the other. And the mediating presence of such a Gestalt would constitute a then and there consciousness of the food or the non-food character of the particular goal presented or represented.

Finally, the switch-over (on any given occasion) from not being ready to treat the sign-relationship of the black and the white as different to being ready to treat them as different would also be due to the mediating function of behavior-adjustments. In this case the behavior-adjustments would be those for actually going down the presented alley and reaching the to-be-expected food or non-food result. Thereby a fused Gestalt would be produced in which the presented stimulus, white or black, not only would be set over against its comparison color but also would be enlarged by its to-be-expected food or non-food result. Only on the basis of this total Gestalt would the behavior of entering or not entering ensue.



To recapitulate, we would suppose the rat's total process of learning to be something as follows: First, the animal, after a greater or smaller number of trials, would come, when faced with the white or the black, to make an adjustment for running back and forth. And he would thereby become conscious of the whiteness or blackness. Similarly, when faced with the food or the non-food, he would make an adjustment for running back and forth and thus become conscious of the foodness or non-foodness. Finally, when faced with the white or the black, he would also make an adjustment to entering that one or the other and thereby become conscious of the to-be-expected food or non-food result. On the basis of all three such adjustments, resulting, let us say, in one grand total Gestalt, he would respond. This total Gestalt would contain the differentiation of white from black, of food from non-food, and of the sign-relationship of white as leading to food from that of black as leading to non-food. And on the occasions of its first appearances there would be consciousness. On later occasions this grand total Gestalt would eventually come by mere associative extension, without the intervention of behavior-adjustments, *i.e.*, without consciousness.

One last word. You will perhaps be doubtful that the lowly rat is capable of all this. So I think am I. The important point is merely that if a rat learns consciously, the above gives a perfectly objective definition of how he might do it. It may be that he learns unconsciously.<sup>2</sup> If he learns unconsciously, then we should have to assume that the changes from the readiness for undifferentiated behavior (*i.e.*, behavior mediated by very simply 'Gestalted' stimuli) to the readiness for differentiated behavior (*i.e.*, behavior mediated by more complexly 'Gestalted' stimuli) occurs somehow automatically between trials. We could then assume no mediating adjustments to introduce these changes. We should be forced to suppose that the initial stimuli somehow grow large and properly 'Gestalted' by mere mechanical accretion.

<sup>2</sup> Though the recently reported results of McDougall and his son (*J. Comp. Psychol.*, 1927, 7, 145-176) tend to minimize the probability of such unconscious learning.

## THE CONSISTENCY OF SEX DIFFERENCES IN MENTAL TRAITS AT VARIOUS AGES

BY FLORENCE L. GOODENOUGH

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It is only within the last quarter of a century that the question of sex differences in mental traits has occupied more than a very minor position in psychological literature. In 1898 the fifth volume of the psychological index for the first time offered a partially separate classification for literature on this subject under the heading of *Individual, sex, and class psychology*. In 1911 this heading was changed to read *Sex, age, and occupational differences*. This wording has been retained to the present time.

In 1903 Helen B. Thompson (Woolley) published her doctor's thesis on the *Mental Traits of Sex* (37). If we except Havelock Ellis' *Man and Woman* (10) which is not of an experimental nature, this is probably the earliest major study devoted primarily to the determination of sex differences in mental traits. A few minor articles had appeared previous to that time, and the question of sex differences had been taken up with more or less thoroughness as a special aspect of several more general studies of mental ability, among which the pioneer work of Gilbert (14) should be particularly mentioned. A study by Thorndike on sex differences in school ability as indicated by promotion records, and by teachers' marks in the various school subjects, also appeared in 1903 (38).

With the development of more adequate instruments for the measurement of mental growth, interest in the question of sex differences in mental ability has increased rapidly, if we may judge by the proportionate number of titles which have appeared in the literature from time to time. A review of the literature which was made by Woolley in 1910 cites twenty-five references (48); a second review by the same writer in 1914 lists eighty-eight (49). A bibliography pub-

lished by Louttit (21) in 1926 which is based upon a relatively small number of sources lists 229 titles of which 151 appeared subsequent to 1914. A more complete survey of the literature would greatly increase the length of this list. A more recent review by Allen (1) lists seventy-four titles, of which only thirty-four were given by Louttit. This review is also very incomplete.

The experimental studies on sex differences in mental traits may for convenience be divided into two groups: (1) those dealing with gross comparisons between the sexes as to total scores earned on composite tests of intelligence, and (2) those dealing with the separate parts or items of such tests or with similar unitary tasks specifically considered.

The first group of studies has been undertaken for the most part in an attempt to throw light upon certain observed differences in the achievement of the two sexes, such as the proportion of each sex who become eminent, sex differences in school progress, the sex ratio in institutions for the feeble-minded, etc. The data have usually been considered both with reference to central tendency and to variability, in accordance with the hypothesis suggested by Thorndike (39) that the greater frequency of males among eminent persons as well as in institutions for the feeble minded might be a result of greater variability of the male sex in regard to intelligence, even though the central tendencies for the two sexes were approximately the same.

The most outstanding impression which one gains from a comparison of these studies is the inconsistency of the various findings. Sex differences are shown, it is true, and while they are in general small, there are still a number of instances in which a sufficient number of cases have been included to give a reasonably high degree of reliability to the differences shown. The direction of these differences varies, however, according to the type of test used, and with the age and school status of the subjects. A few illustrations will suffice.

Thorndike (39) has re-computed certain data taken from Gilbert's early report of tests given to school children (14). He shows that for the greater number of the tests used, the

variability of the boys exceeds that of the girls, though the central tendency of the girls is more often above than below that of the boys. Certain tests, however, show a consistent tendency in the opposite direction; e.g., boys are superior to girls in reaction time, but the girls are more variable; in cancellation of A's or of A's and T's on a sheet of pied text, girls exceed boys both in speed and variability. It is therefore quite possible that a different selection of tests would have led to quite different conclusions. In a study of 453 unselected elementary school children who were given both the Porteus Maze tests and the Binet tests, Porteus (25) reports a fairly marked difference in favor of the boys for both chronological age and Binet mental age groups. On his completion test language scales Trabue (40) finds no differences between the sexes in variability, but the girls exceed the boys in central tendency. In total score on the Pressey group tests, Pressey (26) finds the girls superior to the boys, and the boys somewhat more variable; but both findings are reversed for certain of the subtests. Terman (33-34) reports girls consistently superior to boys in mean performance on the Stanford Binet throughout the elementary school period, and finds no sex differences in variability. In two recent reports from the Institute of Educational Research (29-30), Thorndike and his co-workers have shown that on the I.E.R. tests given to approximately 2,500 boys and girls between the ages of 13 and 18 years the boys slightly exceed the girls both in central tendency and in variability. The difference in central tendency is, in their opinion, sufficiently accounted for by differential selection; the difference in variability, they are inclined to think, may be a true difference, although its small amount and the fact that it is not consistently maintained at all ages leads them to be rather cautious in advancing this opinion. Winsor (47) has assembled the results of a number of studies on the comparative variability of the sexes. He finds that "where a large population of a given age has been tested, the difference in variability is practically negligible. Males have been found to be more variable when some characteristics or types of performance are measured, and



females have been found to be more variable when other characteristics or types of performance have been measured. . . . No justification for the occasional practice of referring to the 'law of greater male variability' as assurance of the trustworthiness of data has been found." Studies by Book (3) and by Colvin and MacPhail (6) show a marked sex difference among high school seniors in favor of the boys in intelligence test scores and an equally marked sex difference in favor of the girls in scholarship grades given by teachers. These findings are corroborated by a study made by Paterson and Langlie (24) who, by means of a supplementary study based upon college freshmen, are able to show that the differences found are largely, if not entirely, attributable to selective elimination of boys from the secondary schools and to non-intellectual factors, such as legibility of hand writing, affecting the teachers' grades which in general tend to favor the girls.

Because of the many factors, both biological and social, which operate unequally for the two sexes either as regards opportunity for achievement or adequate recognition of achievement when accomplished, it is obviously extremely difficult to arrive at any satisfactory conclusion regarding the comparative ability of the two sexes as such ability would be manifested under absolutely equal conditions. The difficulty is not simply a matter of developing adequate methods for the measurement of specific types of achievement, though it should be noted that the methods thus far available are but crude instruments at best; neither is it primarily a question of finding suitable means for unbiased comparison of opportunity, a problem which is far more difficult and toward the solution of which almost no advancement has yet been made. Even were these difficulties overcome, however, there would still remain the infinitely greater problem of establishing some kind of ultimate social criterion against which various types of specific accomplishment, when accurately measured with due correction for inequalities in opportunity, could be evaluated in terms of their true significance for social progress.

It seems probable, therefore, that some time will elapse



before we are in a position to draw any valid comparisons between the sexes in respect to such a complex trait as 'general intelligence.' We turn, then, to the second group of studies previously referred to, namely, those dealing with specific types of performance of a relatively simple nature, though none of these can be considered unitary in a psychological sense. It has been repeatedly shown that while the sex differences on tasks not requiring any appreciable expenditure of physical strength or energy are as a rule small, they are in many instances maintained with a degree of consistency which it is highly unlikely would be the result of chance. Varying interpretations have been placed upon these findings. Many persons have held that they are due entirely to differences in experience, resulting largely from differences in social customs and ideals for the two sexes, while others have been inclined to feel that they can be better explained by the hypothesis that there are minor differences in the patterns of mental growth which are in some degree characteristic of the sexes.

As several writers have been at pains to point out, the practical import of sex differences in mental traits is negligible, since the amount of overlapping is so great that the small differences between the sexes are completely overshadowed by the great variations found to exist between members of the same sex. The question is nevertheless one of considerable fundamental significance for the study of the nature of intelligence and the factors underlying its active manifestations. It is therefore worth while to consider in what forms of intellectual behavior the sexes have been found to differ and the ages at which these differences have been established.

*Use or Comprehension of Speech.*—A slight superiority of females over males in general linguistic ability has been reported by practically all investigators in this field. Mead (22) found that in the group of normal children whom he studied the girls used their first word appropriately about a month earlier than the boys; and among feeble-minded children the difference was approximately two months in favor of the girls. Terman (35), using a slightly different criterion, found that

among children with IQ's of 140 or above the superiority of the girls in age of beginning speech amounted to about 0.75 of a month. Since the mean age of first speech for the sexes combined was 11.34 months for the gifted children, 15.32 months for the unselected group, and 38.52 months for the feeble-minded, it appears that the difference between the sexes in respect to this type of performance is maintained at approximately the same relative magnitude among groups in which the phenomenon is markedly accelerated or retarded as among those in whom it appears at the usual time.

Smith's (28) study of the speech of children of pre-school age constitutes an exception to the usual findings inasmuch as her results show no consistent sex differences with respect to total vocabulary, number of words used per unit of time, or average length of sentence. Her vocabulary data are based upon a vocabulary test given to children over two years, supplemented by vocabulary counts made by parents for a small number of children under two years. The sentence data are derived from one hour samplings of conversation during the free play period in the nursery school. Doran (8) found that girls exceed boys in extent of vocabulary up to the age of five or six years, but after six years the direction of the sex difference seems to be reversed, though his data are not entirely consistent with respect to this point. He is inclined to ascribe the change to the greater freedom permitted the boys and the consequent wider range of information possessed by them. A study now being carried out at the University of Minnesota Institute of Child Welfare by McCarthy shows a consistent superiority of girls over boys in average length of sentence used and completeness of sentence structure, for children between the ages of 18 and 54 months.

Among school children, both in the elementary grades and in high school, a fairly consistent tendency toward a superiority of girls over boys appears whenever the sexes are compared either as to scores earned on tests of a predominately linguistic character or as to school grades on the so-called 'language subjects' such as reading, language or grammar, literature, etc. As regards the test data, the findings of Pyle (27) for

several thousand school children between the ages of 8 and 18 years are perhaps the most representative. He finds a consistent superiority of the girls at all ages on his free association test (number of words which could be given in three minutes), which increases regularly with age. At age 8, the girls do almost as well as the 9 year old boys; at age 10, they are slightly superior to the 11-year old boys; at age 12, the girls almost equal the 14 year old boys; at age 14, their score is equal to that made by the 16 year old boys. Other linguistic tests upon which the girls exceed the boys in Pyle's study are as follows: easy opposites at all ages from 8 to 12 years (test not used for ages above 12), hard opposites in 7 of 9 age groups (ages 10-18 years), word-building in 10 of 11 age groups (8-18 years), and a language completion test utilizing three short anecdotes. The data from this test are especially interesting, since they illustrate the effect of sex differences in interests upon tasks which are apparently similar in type. Selection 1 is a rather sentimental account of a little boy's delight over a gift of fireworks. On this selection the girls exceed the boys in each of the five age groups to whom the test was given. Selection 2 is a fishing story. On this, the sexes do about equally well, the girls excelling in three age groups and the boys in three. Selection 3 is a story of a charitable deed. On this, the girls greatly exceed the boys in all of the six age groups studied. Girls also exceed boys in 8 of 11 age groups on the analogies test. These findings have been in general substantiated by other investigators.

Because of selective factors which operate unequally for the two sexes, it is exceedingly difficult to secure adequate data regarding sex differences at the adult level. It is nevertheless interesting to note that in a study by King and M'Croory (19) of 544 freshmen at the State University of Iowa, the proportion of men who equalled or exceeded the median score of the women was 36 per cent on sentence completion tests and 41 per cent for an easy opposites test; while on a harder opposites test involving many semi-technical words 64 per cent of the men exceeded the median of the women. Paterson and Langlie (24) found that for a selected group of

University of Minnesota freshmen, only 36 per cent of the men equalled or exceeded the median score of the women on the Iowa content examination in English, although 61 per cent of the men of this group exceeded the median of the women in score on the general college ability test. On the other three parts of the Iowa content examination (mathematics, science, and social science) the overlapping of the men ranged from 71 to 78 per cent. Crawford (7) in a study of about 500 freshmen at the University of Idaho finds that on the Thurstone Intelligence Test the men slightly exceed the women in total score, but that this difference is very small in comparison with those shown on the various subtests, in which he finds that "the girls are slightly better in reading, proverbs, and opposites, and overwhelmingly superior in grammar and artificial language. The boys are slightly better in sentence completion, distinctly better in reasoning, and overwhelmingly superior in arithmetic and estimating. . . . Girls enter the university with a better high school record than boys, with about equal general intelligence, but with a different pattern or profile of abilities in which linguistic skills predominate." Crawford also shows that in the entrance test in English the number of errors made by the boys exceeds the number made by the girls by 9.2 times the standard error of the difference.

It seems probable, therefore, that the linguistic superiority of the girls which is first manifested in a somewhat greater precocity of beginning speech development is maintained at a fairly constant level to the time of maturity. Whether this is due to a true difference in the developmental patterns of the sexes which is in some degree independent of differences in training, or whether social forces influencing development in the later years have chanced to coincide with an infantile developmental trait, we are not as yet prepared to say. Nor must we overlook the possibility that very slight original differences in ability may operate as directive agencies through which an entire system of interests, preferences, habits, educational and vocational choices may be built up. Conditions which are themselves transient may nevertheless produce permanent results.



*Number Concepts, Arithmetical Reasoning and Computation.*

—The literature dealing with pre-school children has comparatively little material on the subject of number concepts. Douglass (9) in a study of 40 kindergarten children between the ages of four-and-a-half and six years found a slight sex difference in favor of the girls in his 'dot recognition test.' This difference amounted to not more than three months advancement and in view of the small number of cases may have been purely due to unequal selection. On his 'dot selection' and 'marble recognition' tests, the difference is also in favor of the girls but is smaller than that found for the 'dot recognition test.' All these tests are based upon the ability to estimate numbers without actual counting. The reliability of the separate tests is about .6. Both Hall (16) and the Berlin Child Study Association (42) found boys superior to girls in number concept at the time of school entrance, but their method of determination was less rigidly controlled, so that popular opinion as to the comparative ability of the sexes in dealing with numerical relationships may have influenced the teachers who conducted the examinations. Hartman (17) found no sex differences in number concepts among the 1312 five and six year old children used in his study.

Very little material is available with regard to sex differences in arithmetical reasoning or computation shown in the primary grades. In the Stanford Gifted Children Survey (35), the girls show a slight superiority over the boys both in the arithmetic reasoning and the arithmetic computation tests of the Stanford Achievement Scale at ages six and seven, and on arithmetic computation at age eight. Thereafter, the boys are consistently in the lead. The superiority of the girls at the early ages cannot be accounted for by a difference in general ability, since the mean IQs for the two sexes at these ages differ by less than half a point. Data are wanting to show whether a similar crossing of the curves for the two sexes would be found among larger groups of unselected children.

Studies made in the upper elementary grades and in high schools and colleges show a consistent difference in favor of the



boys on practically all tests involving the use of numbers. Terman (34) finds an overlapping of 33 per cent on the fourteen year test of arithmetical reasoning in the Stanford Binet, of 10 per cent on the 14 year induction test, and 15 per cent on the nine year test of making change.<sup>1</sup> In the sixth grade, Van Wagenen (41) finds a very marked sex difference in favor of the boys in problem solving in arithmetic and a slight difference in favor of the girls in arithmetic computation. The Presseys (26) find grammar grade boys superior to girls on the arithmetic test of their group scale. A like sex difference has been found among high school students by Whipple (46), among college students by Boynton (4), King and M'Crory (19), Paterson and Langlie (24), and others.

It appears from these data that marked sex differences in the ability to deal with numerical relationships are found among older children and adults which have not been revealed with certainty by our present methods of investigation among younger children. This may be the result of inadequacies in method. There is no doubt but that proficiency on simple counting tests and tests of ability to combine small numbers quickly and accurately may be achieved quite readily through rote memory alone, and it is quite possible that such tasks bear little direct psychological relationship to the mathematical problems of later life. It may be that true number concepts depend upon mental traits which are relatively late in appearing, and that their development cannot therefore be profitably studied during early childhood. Or it may be that specialization of ability along mathematical lines in an individual or in a sex is determined largely or entirely by differences in environmental stimulation and social ideals, though it should be noted that Kelley (18) finds the 'nature' factor of greater importance in determining idiosyncrasy in arithmetical computation and arithmetical reasoning than in any other tests included in the Stanford Achievement Scale.

<sup>1</sup> Per cent of overlapping is not used here in the ordinary sense but appears to have reference to the average difference in the percentage of each sex who succeed with the test at different ages.

*Reproduction and Memory.*—Data from a large number of sources show a consistent superiority of females over males in memory tests. In their study of pre-school children, Baldwin and Stecher (2) found girls superior to boys on picture memory and digit span. Terman and Cuneo (36) report a very marked superiority of their kindergarten girls both on digit span and memory for sentences. Among elementary school children, Terman (34) found girls distinctly superior to boys in drawing designs from memory, and in repeating digits either forward or backward. Pyle (27) found girls superior to boys in rote memory for concrete words in 9 of 11 age groups from 8 to 18 years, and in rote memory for abstract words in every age group from 8 to 18. In memory for sense material, the effect of a change in content is again shown as in the case of the completion test previously described. On two of the three selections used, the girls exceed the boys at each age at which the test was given. The first of these is a rather sentimental story of an injured dog, the second describes the unfortunate consequences of a loss of temper. On the third selection, which is the story of a fight between two boys, the boys exceed the girls in five of the six age groups to whom the test was given. On a letter-digit substitution test, the girls were superior to the boys at all ages from 8 to 18. On all these tests, the degree of superiority increases roughly in proportion to the age of the subjects. Pressey and Pressey (26) also found elementary school girls superior to boys on memory tests, Mulhall (23) found girls superior to boys in recognition and recall of words and also of nonsense syllables, but little or no sex difference in memory for geometrical forms where, if anything, the boys were superior to the girls. Gates (12) found elementary school girls unquestionably superior to boys both in auditory and visual digit span and in recognition of nonsense syllables. In a study of college students (11), he found that the women exceeded the men in memory both for digits and for logical material, but the men did slightly better in a substitution test. Another study by Gates (13) compares the sexes with reference to performance on the weekly 'quiz' given in elementary psychology. Results for three successive

years show a consistent superiority of the women on the 'fact' questions which amounts on the average to about  $4\frac{1}{2}$  per cent of the total number of answers given, and an equally consistent though smaller superiority of the men on the 'reasoning' questions. The students were also asked whether they would prefer a quiz involving memory or reasoning. 83 per cent of the women and 57 per cent of the men expressed a preference for memory questions. The same classes were also tested for memory of a short news passage. In immediate memory the women exceeded the men by an average of 4.6 per cent of the amount reported. In delayed memory for the same passage after an interval of from 1 to 5 weeks, the superiority of the women was slightly greater than in the case of immediate memory. In a study of memory for advertising material Strong (31) found that college women remembered 51 per cent more details than college men.

*Amount and Range of General Information.*—Because of the wide variety of material included under this head, adequate information concerning sex differences is very difficult to obtain, and any generalizations based upon the studies thus far reported must be made with considerable caution, since it is always possible that a different sampling of items might yield quite different results. With this fact in mind, it is still worth while to examine the available data in order to ascertain to what extent sex differences may appear in regard to the various kinds of information which have been sampled.

In the study reported by the Berlin Child Study Association (42) which was based upon 2,238 children just entering school, it was found that the boys exceeded the girls in knowledge of approximately three fourths of the 75 'concepts' included. Hall (16) found a similar difference among his Boston children, the boys exceeding the girls in knowledge of 34 of the 49 items considered. In summarizing his results, Hall states that the boys in general excel in number concepts and in knowledge of things not commonly found in their immediate environment; the girls excel in knowledge of the more widely diffused concepts, such as things about the house, parts of the body, etc. Hartman (17), on the other hand,

found that girls exceeded boys in knowledge of 57 of the 100 items which he considered, boys exceeded in 37, while no sex difference was shown with regard to the 6 remaining items. Comparison of his questionnaire with those used by Hall and in the Berlin study shows that a much greater proportion of Hartman's questions have to do with specific objects in the immediate neighborhood, and that the superiority of the girls is manifested chiefly in respect to these items, to such events in nature as hail, thunder, etc., and on religious questions, while the boys excel in knowledge of animals, minerals, occupations, and the like.

Among school children and college students, boys have usually been found to exceed girls both in the so-called tests of 'general' information and in school subjects of a predominantly informational type, such as history, geography, science. Terman (35) reports a marked superiority of boys over girls in total score on a general information test, which is consistent for all ages studied and for both gifted and control groups. Considering the separate parts of the information test, the gifted boys were greatly superior to the girls in science and history, somewhat superior in language and literature, while there was no sex difference in music and art. The control boys were also markedly superior to the girls in science and history, but the girls were slightly superior in language and literature, while no sex difference was apparent in music and art information. Van Wagenen (41) finds sixth grade boys superior to girls in history and geography tests, Pressey and Pressey (26) report a superiority of boys on the information test in their group scale. King and M'Crory (19) find that 61.6 per cent of University of Iowa freshmen men equal or exceed the median score made by freshmen women on a general information test; Paterson and Langlie (24) find an overlapping in favor of the men of 78 and 71 per cent respectively on the Iowa content examinations in science and social science for University of Minnesota freshmen. That the difference is not purely a matter of the specific items included seems to be indicated in a study by Whipple (46), who has analyzed the performance of 834 high school students on each



of the 40 items included under the general information test in Form 6 of the Army Alpha. The girls exceed the boys on, only 4 of the 40 items; *turquoise*, *Scarlet Letter*, *Blanche Sweet*, and *indigo*. The boys excel, not only in knowledge of such conventionally 'masculine' items as *Delco*, *kilowatt*, and 'makings' but also on *Rodin*, *Falstaff*, *Darwin* and others which it is hard to account for on the basis of a sex difference in experience. Terman (35) has shown that the sexes differ considerably in their reading interests during childhood. He summarizes these differences as follows, (p. 448): "Girls are more homogeneous with respect to reading tastes than are boys. Boys scatter their reading over a wider range. . . . As compared with boys, girls read (relatively to total amount read) more than twelve times as many books of home and school life, nearly five times as much emotional fiction, and only a third as many stories of adventure or mystery. Girls care more than boys for fairy stories, and boys more than girls for books of science, history, biography, travel, and informational fiction."

The weight of evidence, therefore, seems to point to the conclusion that boys have acquired a truly wider range of general information than have girls by the time they arrive at the beginning of the grammar school period, and there is some reason to believe that some degree of sex differentiation in this respect may be observed even during the pre-school period. Whether this difference is entirely a matter of social pressure and differences in opportunity and experience, or whether there are more fundamental differences in range and intensity of intellectual curiosity and interests which are in some degree characteristic of the sexes, it is impossible to say. Further investigation of the matter particularly among pre-school children would be highly desirable.

*Mechanical Ability and Motor Skill.*—There is, unfortunately, but little in the data at hand which serves to indicate whether the popular opinion as to masculine superiority in tasks of this sort would be justified if the material used were equally familiar to both sexes. That boys and men on the average do greatly exceed women and girls in the ability to



manipulate mechanical contrivances is not only a matter of common observation but has been brought out very clearly in an extensive investigation on mechanical abilities which has been carried out at the University of Minnesota, the results of which will be published in the near future. Woolley (50) reports that among her working children the only major sex difference shown by any of the tests used was a great superiority of the boys in opening the puzzle box.

It is unfortunate that so few of the reports on the so-called 'performance tests' or form board tests give results for the sexes separately. Such data as are available indicate, as a rule, some degree of superiority of the boys, the extent of the difference varying with the nature of the test. Both Sylvester (32) and Wallin (43) find a very slight difference in favor of the boys on the Seguin form board, Young (51) reports that on the Witmer form board the boys did better than the girls in 15 out of 18 age groups ranging from six years to adults, and that the gross amount of the sex difference increased roughly in proportion to age. On his slot maze (52), Young finds an even greater sex difference, again in favor of the boys, the superiority amounting to approximately one year at the age of four and two years at the age of eight. The difference is present to a marked extent at all ages from four to eight years. An experiment now in progress at the University of Minnesota Institute of Child Welfare shows a similar sex difference in performance on this maze with children between the ages of two and four years. Porteus (25) also reports a well marked superiority of the boys on his series of maze tests. In a ring toss experiment recently carried out by the writer, the average number of successful throws out of a thousand trials made by each child was 168 for 4 four-and-a-half year old boys and 89 for 6 girls of the same age. Only one of the girls did as well as the average boy, only two as well as the poorest boy.

From the data thus far presented, it appears evident that sex differences in mental traits are well established before the end of the elementary school period, and there is reason to think that more adequate methods of experimentation would

reveal equally marked differences of a similar nature much earlier in life. An exceptional opportunity is thereby presented for the study of the origin of individual differences in performance, since there is every reason for believing that factors which tend to bring about differentiation between groups may also operate to bring about similar differences among the individuals constituting the groups. We know that there are certain anatomical and physiological differences between the sexes which are affected little or not at all by any factors which we are normally able to control. It is entirely conceivable that there are similar differences in the reaction patterns most characteristic of the sexes. The fact that these patterns apparently show much overlapping is not to be considered evidence that they are purely the result of accidental experience. A similar overlapping may be seen in very many of the minor or secondary anatomical sex differences, and even 'sex' itself is bimodal rather than completely dichotomous in distribution. On the other hand, we know that social ideals, customs, and opportunities for the two sexes have varied greatly in the past and still differ to a very marked degree. There is much conjecture and perhaps even more display of dogmatic opinion with regard to the effect of these social differences upon such types of performance as have been described, but the amount of sound experimental evidence is regrettably small. As a first step toward clarification of the problem, a thorough study of sex differences shown in infancy and early childhood would be highly desirable.

In view of the small amount of material based upon the examination of young children thus far available, we have attempted to analyze the data obtained from the examination of 300 pre-school children between the ages of 18 and 54 months. The Kuhlman Revision of the Binet Tests and the Wallin Peg Boards, Series A-D, inclusive were used. The method of selecting the cases and the procedure used in giving the tests have been described elsewhere (15). In the case of the Kuhlman, the procedure was essentially the same as that recommended in the manual (20). In the peg board test, the chief departure from the procedure recommended by Wallin

(44) lay in the fact that the boards were always presented in the same order (A, B, C, D) and only one trial was allowed on each.

The children were divided into three age groups of two, three, and four years respectively. Ages were taken to the nearest birthday. Each age group was made up of exactly 50 boys and 50 girls, selected to constitute a representative sampling of the population of Minneapolis by matching the distribution of paternal occupations within each age group against the corresponding distribution for adult males in the city as a whole as reported in the 1920 census. All these children were also given the tests a second time after an average interval of six weeks.

It was pointed out before that sex differences on a composite test such as the Binet should not be interpreted as indicating a true difference in the rate of general mental development of the sexes, since such differences may readily result from a selection of items which tends to favor one or the other sex. The findings are presented, however, both for comparison with the work of other investigators, and because they serve to illustrate the type of error which is likely to enter into material of this kind. The data are given in Table I.

TABLE I

MEANS AND STANDARD DEVIATIONS OF KUHLMAN BINET I.Q. BY AGE AND SEX

	Age 2		Age 3		Age 4		Total Group	
	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls
First examination								
Mean I.Q. ....	104.3	105.9	100.7	108.1	104.7	114.1	105.8	109.4
S.D. ....	13.8	12.0	15.9	19.6	16.2	15.7	15.4	16.4
Second Examination								
Mean I.Q. ....	109.1	107.1	101.9	113.3	112.1	119.9	107.7	113.8
S.D. ....	17.8	12.6	20.2	21.7	14.4	15.3	18.1	17.7

The variations in the mean IQ at different age levels as well as the generally high ratings for all ages are due to irregularities in the standardization of the scale. However, since

the same number of cases of each sex are included at each age, it is permissible to combine the figures for the entire group.

The results are similar to those found by Terman (33) on the Stanford Revision for children between the ages of five and fourteen years, both as to direction and amount of difference. The girls are superior to the boys in five out of the six determinations. The difference between the means for the three age groups combined is approximately twice the standard error of the difference on the first examination, and nearly three times the standard error of the difference on the second examination. The girls also appear to gain more from practice than the boys do, since the difference between the sexes on the second examination is greater than it was on the first. Whatever may be the factors underlying this difference, they appear to be as effective during the pre-school years as they are during later childhood.

Inspection of the separate test items enables us to advance a possible explanation which is at least in accordance with the data presented in an earlier part of this paper. Of the 39 tests<sup>2</sup> from which the scores earned were derived, at least 12 appear to be dependent chiefly upon immediate memory, viz: repeating digits (three tests), repeating sentences (two tests), picture memory (two tests), reproduction of a series of taps (two tests), memory for three simultaneous commands, recognition of forms, and reproduction of paper folding. Giving the family name might also be included in this group, although in this instance, delayed rather than immediate memory is involved. Except for the test of repeating two digits, which shows no consistent sex difference, the girls exceed the boys on all of the tests listed above.

A second source of advantage to the girls is to be found in the predominantly verbal character of the greater number of

<sup>2</sup> This includes all the tests within the range from the point where approximately all succeeded to that at which approximately all failed. The list comprises Test 3 of the 18 months series (beginning speech), all the two year tests except Test 5 (removal of wrapping from food before eating), all the three, four, and five year tests, all the six year tests except Test 6 (counting irregular series of taps), and the tests of picture description, naming coins, and repeating five digits in the seven year series. The irregularities are due to the fact that the order of difficulty of the various tests as found for our group does not entirely correspond with the standardized arrangement.



the tests. Certain tests are obviously almost entirely a matter of linguistic ability, viz: the 18 months test of beginning speech, the three year test of naming objects, and the four year comprehension questions. To these should probably be added the tests of picture enumeration and picture description and the definitions test. The girls of our group exceed the boys on all of these tests. They also excel in pointing out parts of the body and in naming colors, both of which facts are in accordance with the findings of Hall (16) and Hartman (17), in comparison of weights and in the 'patience' test, in aesthetic comparison, in distinguishing morning from afternoon, recognition of mutilation in pictures, and to a lesser degree in naming the first four coins and in tracing a square or an irregular form.

Of the entire group of 39 tests, the boys are consistently superior in only three—imitation of movements, selecting the longer of two lines, and discriminating right and left. None of these requires a verbal response. Six tests show no consistent sex difference. These are: pointing out objects in pictures, obeying simple directions, copying a circle, giving sex, counting four pennies, and copying a square. In these also the verbal element plays a relatively small part.

The general superiority of the girls, not only upon total score but upon so large a proportion of the individual test items, raises the question whether, as has frequently been assumed, the well-known physiological precocity of girls may be paralleled by a corresponding mental precocity. While the hypothesis is plausible, the evidence in support of it must be regarded as far too one-sided to justify such a conclusion. It has been shown that more than half of the total number of items in the section of the Binet under consideration are chiefly dependent upon types of performance in which females have been found to exceed males at all ages for which information is available, while the types of performance in which males have usually been found to exceed females are practically unrepresented. The reason for this apparent differential selection of items is probably to be found in the comparative inconvenience of devising and administering tests which do



not involve the use of language as compared to the rather obvious 'question and answer' tasks or the conventional tests of immediate memory. The ease with which the latter can be handled has led to a general tendency to include an undue proportion of such tests, not merely in the Binet series but in the greater number of the tests commonly used with young children. Until more adequate information is available with regard to the comparative performance of the sexes during the early years on tasks of the type in which males have later been found to excel, we are hardly warranted in drawing any conclusions with regard to their relative precocity in general mental development.

In this connection the results of the peg board tests which were given at the same sitting as the Binet are of interest. Although exactly the same children are included in the group, the general tendency of the sex difference is the reverse of that shown by the Kuhlman. At ages two and three, the boys exceed the girls in total score on the four boards on each of the two examinations given. At age four, the girls are slightly in the lead. While the results are not entirely consistent, they are nevertheless in general agreement with the findings of other investigators, who have usually reported a very slight superiority of boys over girls in tests of the form board type. All in all, the evidence seems to point to the conclusion that *the question of mental precocity as related to sex must be answered in terms of specific functions or traits, rather than in terms of unanalyzed general tendencies.*

The results of this study may be summarized as follows. Mental differences between the sexes are very small in comparison with the amount of variation found to exist between members of the same sex. As far as educational procedures are concerned, they may safely be ignored. Nevertheless, the consistency of the findings of so many independent investigators leaves no reasonable place for doubt that sex differences in mental traits do exist, and in measureable degree. It appears, moreover, that the pattern or profile of abilities which tends to characterize either of the sexes remains relatively constant from early childhood to maturity.

How these differences in pattern have originated is not apparent from the data at hand. They may be due purely to differences in social customs and ideals for the sexes, which have been impressed upon the individual from infancy. On the other hand, these very customs and ideals may themselves have been built up through the operation of minor sex differences in types of native ability or in intensity and direction of emotional drive. From the scientific standpoint, however, the significance of facts such as have been summarized in this paper is far from slight, since they suggest a means by which the origin of mental variations may be objectively approached. A more complete study of sex differences shown in early childhood might do much to clarify our thinking with regard to the essential nature of individual differences in mental traits, and the extent to which such differences may be modified through training.

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## VARIATION IN MELODIC RENDITIONS AS AN INDICATOR OF EMOTION

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Six years or more ago phonograph records of eleven songs all sung by one Picuris Indian were sent to me for transcription from the Smithsonian Institution in Washington. Of these, two songs were practically identical with two others, the melodies having been merely adapted to other words. In order to be sure of obtaining at least one clear and distinct record of each song and to have several repetitions in case of breakage of the record or wearing in the course of repeated playing during transcription, the cautious collector secured from two to five renditions of each of the songs, so that in all there were thirty-four examples. He may also have realized the value for a psychological study of having so many renditions all sung by one person, since a comparison of the different renditions of the same tune would give a fairly good idea of an individual's range of variability in performance. My reason for transcribing all the examples, however, was merely to enable me to designate to the collector the rendition which seemed the least irregular or imperfect, musically speaking. Text imperfections I left for him to discern and thought that from these two criteria he might settle upon the most satisfactory version of each song for publication.

They were totally unfamiliar melodies. In writing out the different renditions of a single tune, each was done independently and with the utmost care. I made no reference to the others either by visual comparison of the notation or by musical memory, and consciously endeavored not to fall into the practice of standardization, having long before realized the danger of that. Each transcription was tested again and again for the slightest inaccuracies in representing



itches and rhythms. It was impossible at the time to make an analytical study of the songs, and without even comparing the various renditions of each song with one another except to note which seemed the best, the transcriptions were submitted. A few weeks ago the songs were sent to me with a request for an analytical study of the music to add to the ethnological report in which they were to appear. It had been decided to print all the versions, thus avoiding any arbitrary selection.

The scales derived from the tunes and their various renditions proved to be quite interesting. The method of calculating them was as follows. The total number of time beats accruing to each pitch in each tune was calculated by adding up the time value of all the notes falling on it. That pitch which was struck most frequently, and thus consumed the largest number of measure beats, was taken as the standard in each song and given the value of 100. In writing the scale the denomination of this note was written as a whole note. The percentage value of each other pitch in relation to the standard tone was then calculated and assigned a note value which would most nearly represent it. By using dots and double dots the exact values could be approximated quite closely in musical notation, at least as closely as was necessary, since the main object in writing the scale in different note values was merely to afford a means by which the varying and relative importance of the different pitches could be seen at a glance. It offered a much better graphic presentation than writing the scale in notes of one denomination accompanied by their percentage values. Lest this crude representation might not satisfy the more exacting critics, the actual number of measure beats accruing to any pitch was written above the note representing that pitch in the scale, from which anyone might readily calculate its percentage value in relation to the rest. In endeavoring to ascertain what are the prevailing scales in a given type of exotic music, it is misleading to the analyst as well as to his readers to fail to note which tones are structurally important in the melody and which partake of the nature of ephemeral

or embellishing tones. The dividing line between these I took at about the sixteenth note values (in my derived scales). Tones which appeared less than  $1/32$  as many times as the principal tone were written without stems and considered merely as ephemeral.

The pitches occurring in each song were ranged in order from highest to lowest. It was found that the 'standard' note did not preponderate over the other tones by merely a few beats, but that usually its value was several times as great as that of any other note in the scale. In some cases its value was greater than, or about equal to, the sum of the values of all the other notes. In about half of the songs it was the lowest note and for a large number it was the final tone as well. From these considerations it seemed clear that this note stood in some fundamental relation to the song as a whole. In order to better compare the individual scales of each song which lay at various pitch levels the 'standard' or principal tone of each scale was regarded as the pivot and transposed to the *c* pitch and space on the staff, the rest of the scale being accordingly transposed to its original intervallic relationship to the principal tone. The scales were then arranged in groups according to the degree of their similarity. It was seen at once that those derived from the different renditions of a single tune were, as might be expected, most closely related, and that the scales of all the songs fell into five groups between which there were fundamental differences. There were two groups in which the tonality of the scales was markedly major, that is, the scale tones having the largest values occupied diatonic positions in relation to the principal tone, including the presence of a pure major third and sixth, while the minor third and sixth were absent altogether or represented merely in what might be called ephemeral tones. Intermediate pitches between some of those tones occupying diatonic positions were present, but only in tones of comparatively little value. These intermediate tones occurred in fairly constant locations in the range of the scales of any group. The only considerable difference between the two groups of scales which were

markedly major in tonality was in the position of the 'standard' tone in regard to the rest of the scale. In one group it was the lowest tone, in the other it occupied a middle position and the scale ranged from the dominant above it to the lower dominant, or in the neighborhood of those tones.

Two of the other groups were markedly minor in tonality, or if the major third and sixth were present, they were less prominent than the minor intervals. These two groups were distinguished by a difference comparable to that between the two major groups, that is, it consisted in the position of the 'standard' tone which in one group was at the bottom of the scale range and in the other occupied a middle position.

The fifth group was composed of the scales of three songs having a very limited range of about a third, in which the principal tone was the highest note and the next prominent tone lay about a whole step below. Thus the true seventh, or leading tone, which in major and in some minor scales is closely associated with the fundamental, was very inconspicuous in these three tunes which were difficult to classify as to tonality.

Having established the fact by the structural tones that most of the scales were either major or minor in relation to the principal tone as fundamental, it remained to examine the intermediate ephemeral tones. It has already been stated that these occurred chiefly in certain locations, and practically always between two adjoining diatonic scale tones, both of which were fairly prominent in the tune. They did not seem to take the place of missing diatonic tones, but to occur in addition to the 'true' pitches, if this adjective may be allowed. In other words, they appeared to be additional to the diatonic tones as deflections of them. Although in different songs they occurred in several localities, it was noted that they were often present between the dominant and the subdominant above the fundamental, so much so that at first it seemed as if the tri-tone were being evolved although a true augmented fourth was comparatively seldom touched. An examination of the songs themselves, however, showed that this and other fairly constant situations of the

ephemeral tones were more the fortuitous result of other circumstances than of a groping for the tri-tone, or of substituting intermediate for diatonic pitches in the main structural scheme. A most interesting situation was revealed. In the first songs, which were numbered in the order in which they were taken by the collector and in which they occurred in the myths to which they belonged, the intermediate tones practically all lay at the maxima of up or down trending melodic curves. They appeared to be deflections of the diatonic tones which would normally belong there and which in other situations in the melody were not 'deflected.' The 'deflections' seemed to denote a tendency to melodic attenuation; in other words, the singer failed to reach the logical maxima of the up or down trending melodic curves. Practically all of the intermediate scale tones which occurred in the first songs were to be accounted for in this way. There were very few if any instances where the 'deflected' pitches might have been considered as the result of over-singing an interval or acuating a curve. The situation was quite different, however, for some of the later songs, and particularly in all the renditions of the last of the eleven songs constituting the myth cycle. As the end was approached the tendency to attenuate melodic curves appeared to a more limited degree while an increasing tendency to acuate curves was observed. In the renditions of the eleventh song the first tendency gave way entirely to the second.

This situation, together with considerable experience in observing not only the singing of so-called primitive peoples but of trained singers of our own race, and with my knowledge of the conditions of field collecting, particularly of the methods of the collector of these songs, set me to speculating as to the cause of this phenomenon. It seemed likely that the final five renditions of songs were given when the singer was becoming increasingly irritated or tired. His earlier songs had betrayed what might almost be assumed to be a relaxed condition, a carelessness in melodic enunciation, a lack of attention to details, or possibly a phlegmatic temperament. But that the man possessed a phlegmatic tempera-



ment seemed doubtful in view of his manner of singing the later songs. I felt sure that he had entered on the really exhausting task of dictating in his own language the myths in which the songs belonged, translating the texts, answering involved and difficult questions of grammar and singing the songs, with some pleasurable anticipation which might have been increased by the prospect of generous pay. On the other hand, I knew that the collector was a hard worker whose own enthusiasm did not wane even when his strength was exhausted by long hours of concentration, even lasting far into the night. I knew that he would ask a question again and again until he had satisfied himself that he had not misinterpreted the answer and that under such grilling and concentration along unaccustomed lines the Indian, despite his natural courtesy, becomes tired and impatient. In order to satisfy myself as to these points I therefore put the following questions to the collector, without giving him my reasons, except to say that some problems had come up in studying the songs that might have a psychological interest.

1. Can you recall whether you took the records of the songs all at once, *i.e.*, all at one sitting, or did you take them at intervals as you got to the songs in recording the myths?

2. Did you take all the renditions of each song at one sitting and in close succession or did some time elapse between the different renditions?

3. If you took the songs and their various renditions all at once, can you recall if the man seemed tired toward the end, or can you recall any incident when you took the last song, the Bluejay's Grinding Song, that might have upset his usual ease?

4. Did he seem to be of a phlegmatic temperament or was he easily upset?

5. Did you work with him over long stretches, and were you working with him at night on the last songs?

His reply was as follows:

Your special delivery letter is this moment received and without delay I will answer the interesting queries.

1. I took the records at intervals far apart, not at one sitting but as I got to them in recording the myths.

2. I did not take all the renditions of one song at one sitting or in close succession, but some time elapsed between the different renditions.

3. The informant would often get very tired from dictating the difficult stories and out of patience with the slowness of phonetic writing. I remember that he said that the Bluejay's Grinding Song is a woman's song. His memory of it was probably aural rather than one of mouth and throat movements. Some of the songs he seemed to take a delight in singing, *e.g.*, the songs of the Elf. But not so with the Bluejay's song and in fact, several of the later songs. The phonograph also disconcerted him somewhat.

4. He was not of a very phlegmatic temperament for an Indian but rather easily upset. He enjoyed parts of the work greatly. In other stretches he seemed quite annoyed by it.

5. I worked with him over long stretches and had no mercy on him. We interspersed the stories with all kinds of grammatical questions, which pulled his mind from one thing to another in rapid succession. While the investigator looks for similarities in phonetics, the informant is often bewildered by the apparently (to him) disconnected material asked.

At first glance the answers to my first two questions might seem to reduce the probable correctness of my conclusions, but if the Indian was being held to his work continuously, as the whole letter seems to indicate and as my knowledge of the collector's methods leads me to assume, the fact that he was not singing continuously does not mean that he was not being examined and made to fix his mind on phases of thought to which he was unaccustomed. When an Indian has once given what he thinks to be a complete and honest answer to a question he is always considerably nettled at being asked to repeat his reply, or at having the same question put to him in another way, whether it be a question on language, on story, or a request to sing a song again. It is significant indeed that the song which in all its renditions showed the greatest tendency to acuation of the melodic curve should have been a woman's song. I do not think the aural memory rather than one of mouth and throat movements had anything to do with this peculiarity of singing. The words of the song were no doubt all contained

in the ordinary vocabulary of the tribe and he had probably used them countless times in other connections, while the melody in other respects than acuation had nothing unusual about it. The explanation lies in the fact that it was a woman's song and no normal Indian man wishes to be distinguished from his fellows by doing an unconventional thing, and especially by being associated with what are properly women's affairs. Probably this was the last straw after a long series of small irritations which it was difficult to conceal.

A falling tone is generally understood to imply finality or emphasis. Therefore the question might be raised, does a deepening of the trough of a downward trending melodic curve mean the same as the sharpening of the peak of an upward curve? Probably the elements of finality and emphasis are present in the tendency to acuate downward curves, and emphasis, if not finality, in the acuation of upward curves. But it seems to me that more is involved in the acuation of curve troughs than in the mere dropping of tones near the close of a song. A second question which comes to mind is whether such over-emphasis of melodic contours in singing really does indicate irritation or excitement in the singer, while a tendency to smooth their sharpness follows a certain relaxation even such as might come from pleasure as well as from indifference. It seems probable that both of these questions may be truly answered affirmatively. At any rate an hypothesis as to the probable correctness of these assumptions may be framed on the basis of the evidence afforded by the performance of this individual. An interesting problem is suggested which might repay investigation. Possibly such tendencies in singing, if generally true for the human race, might serve as another useful means of testing states of emotion.

#### SUMMARY

1. A collection of eleven different songs totalling thirty-four repetitions sung by one Indian afforded excellent opportunity to observe variation in performance. No two rendi-

tions were identical, rhythmically or melodically, but the range of variation did not exceed normal, possibly excepting one peculiarity.

2. At first, when comparatively fresh and singing songs that he liked, the singer betrayed a marked tendency to attenuate both up and down trending melodic curves, involving both sharpening and flattening. Later the tendency toward attenuation lessened distinctly in favor of acuation of the curves as the singer became tired, and especially when singing five repetitions of the final song which he disliked.

3. It is possible that these tendencies toward melodic attenuation and acuation are unconscious expressions of the emotions of pleasure and irritation and that acuations of upward melodic curves imply the same emotional situations as acuations of downward melodic curves. The data suggest the possibility that such phenomena may be observed in other groups in connection with emotional disturbance.



## EQUIPMENT FOR THE TEACHING OF PSYCHOLOGY<sup>1</sup>

BY JOSEPH PETERSON

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It is illegal for a physician to practice medicine without a license. Recently there was an outburst of public indignation against the fraudulent certification of medical practitioners who had not had the standard training for the work which they were doing. Elementary teachers in public schools must be certificated by state or county officials to whom we entrust the responsibility of the training of our children. They cannot get their pay without having been properly certificated, even though they work under more or less close supervision by experienced educators. Such authorization for teaching is not required in normal schools and the various types of colleges which are 'lending a hand' in the education of our future citizens. The boards of state institutions employ a president to whom they entrust the responsibility of selecting properly qualified instructors. In the universities and better colleges the faculty members are usually employed upon the direct recommendation of recognized specialists under whom they received their training, and on the basis of certificates in the form of degrees issued by university authorities; but the practice is far different in the poorer grades of normal schools and colleges, whether public or private. Indeed the condition here is in serious need of investigation.

A recent report by the School Service and Research Bureau of the University of Kansas reveals a grave deficiency in the preparation of teachers in the high schools of the state in certain subjects which ordinarily are the hardest to teach and which represent not more than one or two semesters of work in the high schools. "It is a situation which merits earnest

<sup>1</sup> Read in a preliminary form in the Philadelphia meeting of the American Psychological Association, December, 1926.

consideration," we are told, "that in all three groups of schools [those employing not more than ten teachers, those employing from eleven to twenty-nine, and those with over thirty teachers, comprising groups I, II, and III, respectively] the teachers who have the least preparation in the subjects taught teach sociology, economics, physiology, psychology and physiography. Since none of these five subjects represent more than one or two semesters of work in the high-school curriculum, it is more easy to understand why so few teachers are well prepared in them and why schools have not more generally demanded adequate preparation in these subjects when employing those who are to teach them. However, no explanation of how the situation has come to exist can really excuse the injustice which is likely to result to those students who are assigned to teachers that are obviously not prepared for the work. A most likely consequence is that under such circumstances the instruction will be characterized by enslavement to a textbook and that formalized memorizing of unvitalized facts, without much emphasis on their functional value, will frequently prevail."<sup>2</sup>

With respect to psychology, in which we are here especially interested, one table shows that 86.5 per cent of 189 teachers of this subject have had only five semester hours or less preparation in psychology, 40 per cent having had even less than five hours. Those of us who know even the college student who has had only the first course in psychology can imagine the preparation of such teachers of psychology! The really tragic thing about this situation is that "practically all of the psychology" is "taught to meet a requirement in the normal-training courses which are offered in the high school for the preparation of rural-school teachers." (p. 18.) Who would not say: Better have no psychology taught than such training in the subject? In this regard Kansas is probably not worse off than many other states, whether Northern, Western, or Southern. Is it any wonder that many educators, who have been trained this way, and who associate with teachers thus

<sup>2</sup> Obrien, F. P., 'The High-School Teaching Load and Preparation of High School Teachers,' *Kansas Studies in Educ.*, 1926, Vol. 1, No. 5, pp. 17 and 18.

trained, find psychology of little aid in education? Data on hand in the present study reveal the fact that many teachers of psychology in normal schools and junior colleges are but little better prepared for their work than are these high school teachers.

For some years the chief obstacle to effective work in advanced courses in psychology in the Jesup Psychological Laboratory has been the inadequate preparation of students presenting psychology credit from various other institutions. Even though each student is admitted to any course only after a personal interview with the instructor, we have found it necessary in all advanced courses to review work that should have been covered in the introductory courses, and to spend much time giving special aid to weak students. My own experience in other institutions has not been very different. A preliminary investigation of the teaching of psychology in certain Southern state institutions was consequently undertaken a couple of years ago, and the results were reported to the Southern Society for Philosophy and Psychology at its Lexington meeting in April, 1926.<sup>3</sup> With a few notable exceptions the thirteen state institutions then included in the investigation showed a deplorable condition as to instructors' preparation and as to general equipment for the teaching of psychology, the normal schools and teachers colleges standing out conspicuously as a class in their inferiority in this regard. Teachers colleges offering degrees based on four years' work had not perceptibly improved their faculties and their laboratories and library facilities for the teaching of psychology over the conditions which obtained when they were mere normal schools, if we may judge from a comparison with present normal schools. The teachers colleges and normal school reporting in the first investigation (and they were doubtless superior to those not reporting) showed no appreciation of the fact that psychology is a science and can be taught effectively only where there are laboratories, journals, and instructors trained in the science. "Most of them even of collegiate rank," to quote from the original report, "have no laboratory

<sup>3</sup> See report and the resolutions of the Society in *PSYCHOL. REV.*, 1926, 33, 385-396.

equipment and no funds for any, no journals, few real psychology books, and no plans to improve in these regards. The majority of them do not even have teachers for psychology who have been trained in psychology as far as to the requirements of the master's degree. Indeed, the majority have not even done their major work in psychology, and evidently have no professional standards as to teaching in lines outside the range of their preparation" (p. 393).

After a full discussion of the report, the Society passed resolutions and appointed a committee to coöperate with the institutions toward an improvement of the unsatisfactory conditions revealed. The resolutions, adopted by an apparently unanimous vote of the large percentage of the membership present, reads in part as follows: "Be it resolved . . . that the Society expresses its disapproval of the teaching of the first year's courses in psychology, educational psychology, and other branches of psychology, by anyone who has not had the training represented by at least a master's degree with major in psychology, and also of the teaching of advanced courses in these subjects beyond the first year's courses by anyone who has not had at least the training equivalent to a doctor's degree with major in psychology; that the Society asks all heads of departments in which these subjects are taught, whenever the question of transfer of credit from other institutions to theirs is raised, to disapprove the transfer of any credits in these subjects not taught by persons as indicated above " (*op. cit.*, p. 396).

The committee of five appointed by the Society proceeded at once with the work assigned them. An explanatory letter and certain blank forms, to be filled out by instructors in psychology, together with a reprint of the report, including the resolutions of the Society, were sent out to the presidents of about 420 institutions in the sixteen Southern states and in the District of Columbia. These institutions included all those listed in the 1926 *Educational Directory* as state or municipal universities and colleges, normal schools and teachers colleges, junior colleges, and also all private institutions in these same classes, whether denominational or independent.



Since it had been found in the preliminary investigation that the training of the instructor is most significantly revealed by the degrees awarded him and the persons under whom his major work was done, this information was sought from every institution, as well as information pertaining to laboratory and library equipment. In short, each institution was asked for the names of all courses in psychology which it offered and for such data on its equipment for teaching those courses as could be furnished by the instructors with very little effort and loss of time. (See Appendix for copy of letter and data sheets sent out.) In the letter referred to above, which was sent to the presidents of the institutions early in November, 1926, the following statement is made: The committee "proposes to publish for the Society and to distribute to registrars and professors of psychology in all our higher institutions of learning, particularly but not exclusively in the South, a list of all courses offered in each normal school, college, and university in our own territory that meet the standards defined by the Society in its 1926 meeting. . . . Your institution will receive a copy of the approved courses, listed by institutions, alphabetically arranged, when this report is printed. Courses approved will be those which seem to meet the standards of the Society for transfer of credit and on which advanced work may be based." The committee has thus obligated itself to do a rather gigantic and responsible piece of work, but one which if successfully carried out will doubtless be of inestimable benefit to the South; and its members, as representatives of the Southern Society, desire to assure all institutions concerned of its sympathetic interest in helping them overcome conditions which are certainly not desired by anyone. Moreover, the committee believes that it is in a position to help very markedly any institution needing assistance along these lines.

To what extent can we expect the coöperation of the various institutions in this undertaking? There is a well known tendency in human nature to hold back and even to use evasive means of response whenever there is danger of exposing one's own or one's group or institution weaknesses.

Motives toward kindly coöperation are often misinterpreted. We find evidence, of course, of such concealing responses in some of the replies to our requests for data; and undoubtedly most of the failures to reply are 'responses' of this kind. But the affair is not a personal or private one. Numerous institutions are advertising courses in psychology which they are totally unqualified to offer, and they are thereby inducing hundreds of young men and women to spend time and money in efforts that are worse than wasted. The results are sure to be bad in at least two senses. First, the students are deceived by failing to get the training that they seek and for which they pay, and often learn only when they go to higher institutions for more advanced training that they have lost their time and means and are really not prepared for advanced work. Their disappointments under such circumstances are sometimes grave. Secondly, the insipid text-book content—usually derived from out-of-date books—which under the circumstances is taken uncritically and passively and memorized for examinations, creates a dislike for psychology and produces a scarcity of workers in a field of science that has doubtless very great promise for the betterment of mankind. The ignorance in the popular mind of the most elementary facts of human behavior—its motivations and means of modification, as well as its possible enrichments by the establishment of controls toward long-ranged results—is certainly at present very great, and the field for illuminating this realm of nature is most inviting. The progress that has been made by physical science toward the enrichment of human life can probably be matched by the biological and social sciences, including psychology. Science must discover to the popular mind the natural laws operative in all aspects of behavior.

Within less than five months after sending out our letters to the various institutions indicated above, 158 institutions had sent their data to the chairman of the committee. This is approximately 38 per cent of the institutions circularized, and indicates that the work of the committee is being taken seriously. This conclusion seems warranted from the fact

that the institutions replying include the most important non-sectarian, private institutions in the section studied, and most of the state universities and of the better teachers' colleges.

Table I shows the percentage distribution of 302 instructors in psychology with respect to the highest degree obtained by each and the major line of work done in training for the degree. It will be noted that of these 302 psychology teachers

TABLE I

SHOWING THE DISTRIBUTION OF INSTRUCTORS IN PSYCHOLOGY AS TO THE HIGHEST DEGREE OBTAINED AND MAJOR LINE OF WORK IN GRADUATE TRAINING

Major Department in Training	Number of In- struc- tors	Highest Degree Obtained							
		Ph.D. or Ed.D.		M.A. or M.S.		B.A.		Degree Not Indicated	
		No.	Per Cent of All	No.	Per Cent	No.	Per Cent	No.	Per Cent
Psychology.....	63	40	63	20	32	3	5	0	0
Educ. Psy.....	22	7	32	15	68	0	0	0	0
Philosophy.....	23	9	39	12	52	2	9	0	0
Education.....	111	25	23	69	62	15	13	2	2
History, Political and Social Science.....	16	2	12	10	63	4	25	0	0
Theology.....1	22	3	14	11	50	8	36	0	0
Biblical Lit.....2									
Religious Ed.....1									
Language.....8									
Biology.....3									
Mathematics.....1									
Physical Sciences...4	45	5	11	18	40	5	11	17	38
Agricultural Ed.....2									
Dept. Not Given.....									
Total.....	302	91	30.2	155	51.3	37	12.2	19	6.3

the percentage of those with the Ph.D. degree having had their training in psychology is very much larger than that of those having done major work in any other field. In other words, those instructors who have specialized in psychology in their training have received much more academic instruction than have those with major work in other lines but who are nevertheless attempting to teach psychology. While but less than

four per cent of the 85 instructors trained specifically in psychology and educational psychology have only a bachelor's degree, over 18 per cent of the 149 instructors trained specifically in such lines as education, history, social science, Biblical literature, English, biological and physical sciences, mathematics, agricultural education, ancient and modern languages, theology and religious education attempt to teach psychology when they have only a baccalaureate degree. Over sixty per cent of these 149 instructors trained in other lines than psychology but now teaching psychology, as compared with 41 per cent of those specifically trained in psychology and educational psychology, have only a master's degree. One seems justified, from the data shown in this table, in concluding that the further a person's training is away from psychology the more willing he is to teach psychology with only a very little academic training. The situation is, however, much worse than the figures in the table show: for 15 per cent of the 302 instructors heard from did not indicate the line of their training at all, and probably one-half of the teachers of psychology in the 420 institutions investigated had not replied at the time of this writing. It is safe to assume that those who are trained in psychology are the most willing to reply to our inquiry and to coöperate toward the betterment of the deplorable condition revealed in our previous study. This assumption is supported by various lines of indirect evidence: (1) Most of the active members of the Southern Society for Philosophy and Psychology are among the minority who have replied. (2) The desire to show fit training in psychology is evident in the replies, several of the instructors giving their *major* subjects of academic training as 'education and psychology,' 'philosophy and psychology'; etc., some even mentioning three lines as a major subject; and the institutions named as conferring the degree, and the major professors given, make it clear that psychology could at best, in these cases, be but a minor subject. (3) Persons with higher degrees who are not known to the active membership of the Southern Society, frequently omitted mention of their lines of training and especially of the names



of their major professors.<sup>4</sup> The denominational schools lead in this kind of evasion.

It is to be noted, finally, that 37 per cent of the psychology instructors replying had had their major training in education, and that of these 77 per cent have training only equal to or below that represented by the master's degree in education. It is most unfortunate that these individuals, often so poorly trained in experimental technique and in scientific methods generally, assume so large a responsibility for the teaching of psychology in our poorer institutions of higher education. Psychology in its modern aspects can be successfully taught only by specialists.

Of the 158 institutions replying, 100 are private (about 75 per cent of these being sectarian) and 58 are public, or state supported. In the former group at least 28 per cent offer psychology and 45 per cent educational psychology in the department of education, while 28 per cent do not report on this point regarding psychology and 22 per cent do not give the relevant data on educational psychology. Of the 58 public institutions 21 per cent offer psychology in the department of education, and 7 per cent make no reply to this question; as to educational psychology the corresponding percentages are 38 and 10. The most important institutions, whether public or private, have separate departments of psychology. It is questionable whether psychology can have any real development as an appendage of the department of education. At any rate, no example of such a condition is found in America today. This should be sufficient evidence to any institution of the need of a separate department of psychology.

<sup>4</sup>In justice to several instructors who frankly revealed their personal unfitness for the work assigned them, or the impossibility of getting necessary equipment and journals, it must be said that the tendency to emphasize fitness was not universal. A number of personal letters express appreciation of the work of the committee and say that it has already helped them in their efforts to improve conditions, or that it will help them.

Table 2 gives the approximate number of psychology books and the number of periodicals devoted to psychology reported as available in the libraries of the 158 institutions, classified here also as public and private. It is probably safe to say that institutions naming no periodicals have none, or at least not more than one or two. It appears, then, that about 50 per cent of the private (in nearly all of these cases sectarian) institutions have no psychological journals, and that 65 per cent have not more than one. The corresponding per cents for the public institutions are 26 and 34.

TABLE II  
NUMBER OF PSYCHOLOGY BOOKS AND PERIODICALS REPORTED IN THE  
LIBRARIES OF PUBLIC AND PRIVATE INSTITUTIONS

Books			Periodicals		
Number	Frequency		Number	Frequency	
	Public	Private		Public	Private
1000 and more.....	9	6	20 and more.....	7	3
500-999.....	4	9	15-19.....	4	1
250-499.....	8	12	10-14.....	0	1
100-249.....	24	31	5-9.....	7	8
50-99.....	5	11	3-4.....	11	10
25-49.....	3	8	2.....	9	12
0-24.....	0	7	1.....	5	10
Not Given.....	5	16	0.....	1	7
			Not Given.....	14	48
Totals.....	58	100		58	100

Many colleges of secondary grade report both courses and journals that are not at all concerned with, or to be classified under the captions, psychology or educational psychology. They fail to recognize the status of psychology as an independent science; but what is more important, they probably do really not know what modern psychology is! They have no instructors trained in psychology, no laboratory facilities, and no journals to keep them in touch with the productive centers of research in this line of work—and they contribute nothing whatever to the literature of psychology. Would it not be

far better for such institutions to advertise themselves as offering no work in psychology for the present? Such a course seems to be the only way open from the standpoint of honesty and fair dealing to students, and it would probably be the best way of eventually getting the needed funds for instructors that are properly trained and for the necessary laboratory and library equipment. Such action on the part of these institutions would surely be no loss to anyone whom the institutions are to serve.

It is highly detrimental to any institution to feel that it has no responsibility for the advancement of knowledge and methods of investigation in the lines of courses which it attempts to offer; that its sole function is to 'teach' these subjects. A natural consequence of such an attitude is dogmatic teaching and even indoctrination, the uncritical promulgation of theories and views as ends in themselves, and the mere memorization of text-book statements. Instructors who come to their classrooms not with the inspiration of fresh research problems and new technique (which impress students with the limitations of our present knowledge and give them a taste of the joy of participation in the fine big game of exploration along with the fraternity of original investigators), but with their heads crammed full of recently reviewed dry facts and dogmas to be recited from text-books, cannot be expected to create an interest in the science and to recruit it with new workers. Neither can they be expected to 'give the psychology of' anything in a manner to be of much aid to the practical teacher in our elementary schools. Institutions which are training teachers are probably even more in need than other colleges or universities of real research work in psychology, despite the fact that most of them, by their practices, seem to hold the contrary view. The right of a president of an educational institution, whether public or private, to advertise science courses and then to use them for the promulgation of dogma, affording no opportunities for real scientific work, may well be challenged. It is highly important that scientific bodies set standards in their respective sciences and define the minimum qualifications of

instructors and the minimum facilities for the teaching of these sciences. What other bodies of men are able to do this? Recent revelations of popular ignorance of the methods and facts of the sciences probably warrant the conclusion that most deficiencies in equipment and personnel for the teaching of psychology and of other sciences (which are probably in a similar situation) are due to sheer ignorance. But the harm to society is nevertheless very great.

It is doubtless impracticable to keep up to date a list of psychology courses which meet standards set up, say by the American Psychological Association, or by the Southern Society; because there are constant changes made in the faculty personnel of higher institutions of secondary rank. Moreover, there are often several instructors offering different sections of the same course, some of whom are qualified and some of whom are not. In cases like this our committee will probably specify whose sections are approved. It seems, however, to be practicable and highly desirable that such scientific organizations not only define standards but refuse to recognize work not meeting the standards. In the transfer of credits and in application for state certification the burden of proof, in the light of present conditions, may well be placed on institutions which recommend credit. The constant activity of vigilant committees is also to be recommended. Some sort of action on the part of the American Psychological Association along lines suggested by the experience of the committee of the Southern Society for Philosophy and Psychology would doubtless give much aid toward the improvement of conditions in the teaching of psychology, which are certainly bad, and it might accomplish much toward the recruiting of scientific workers in the various aspects of human behavior. It is possible for committees of scientific associations to make suggestive outlines of content of various courses; to list inexpensive apparatus; to keep up-to-date lists of references and journals; and possibly even to secure helpful discounts in subscriptions to journals by individuals, clubs, and institutions; to prepare lists of properly qualified candidates for teaching positions in psychology, specifying



the nature of the work for which each candidate is prepared; and to help struggling departments and institutions in many other ways. Such activities would greatly increase the circulation of our psychological journals and extend the influence of research in psychology.

## APPENDIX A

GEORGE PEABODY COLLEGE FOR TEACHERS  
NASHVILLE                      TENNESSEE

JESUP PSYCHOLOGICAL LABORATORY

October 30, 1926.

President .....,

Dear President .....

Under separate cover I am sending you a copy of a report on the teaching of psychology and educational psychology, which shows "a deplorable condition," to put it in the words of a prominent American psychologist. The Southern Society for Philosophy and Psychology is desirous of giving its assistance toward the improvement of any unsatisfactory conditions within its own territory, and, as you will see in the report, has appointed a committee for this purpose. This committee desires to obtain as complete information as possible regarding every course in psychology and educational psychology offered in this region in both state and private institutions. It proposes to publish for the Society and to distribute to registrars and professors of psychology in all our higher institutions of learning, particularly but not exclusively in the South, a list of all courses offered in each normal school, college, and university in our own territory that meet the standards defined by the Society in its 1926 meeting at Lexington, Kentucky. The members of the committee believe that this action will be of inestimable value to education in the South, and earnestly ask your co-operation.

Will you, therefore, with the aid of your instructors in psychology and educational psychology, fill out the blanks in the accompanying sheet and return the sheet in the enclosed stamped and addressed envelope at your earliest convenience? Your institution will receive a copy of the approved courses, listed by institutions, alphabetically arranged, when this report is printed. Courses approved will be those which seem to meet the standards of the Society for transfer of credit and on which advanced work may be based.

Thanking you in advance for your co-operation, I am,

Yours very truly,

(Signed) JOSEPH PETERSON, *Chairman*  
(for the committee).

## APPENDIX B

(Fill out for Psychology.)

Name of Institution..... Blanks filled out by.....

City..... State.....

Nature of Institution: State.....; City.....; Private..... If  
church, give denomination.

In what department are courses in Psychology offered?

Courses offered in Psychology:

Title of Course	Hours Credit	Lab. Hrs. per week	No. of Weeks	To whom offered? (show by check)					Name of Instructor
				Fresh.	Soph.	Jun.	Sen.	Grad.	
1.....									
2.....									
3.....									
4.....									
5.....									
6.....									
7.....									
8.....									
9.....									
10.....									
11.....									
12.....									
13.....									
14.....									
15.....									

Courses offered (cont'd)

Instructor's degrees with dates	Institution granting degree	Major Dept. for Degree	Allied Work	Major Professor	Other Profs.	Chief pub- lications	Date
1.....							
2.....							
3.....							
4.....							
5.....							
6.....							
7.....							
8.....							
9.....							
10.....							
11.....							
12.....							
13.....							
14.....							
15.....							

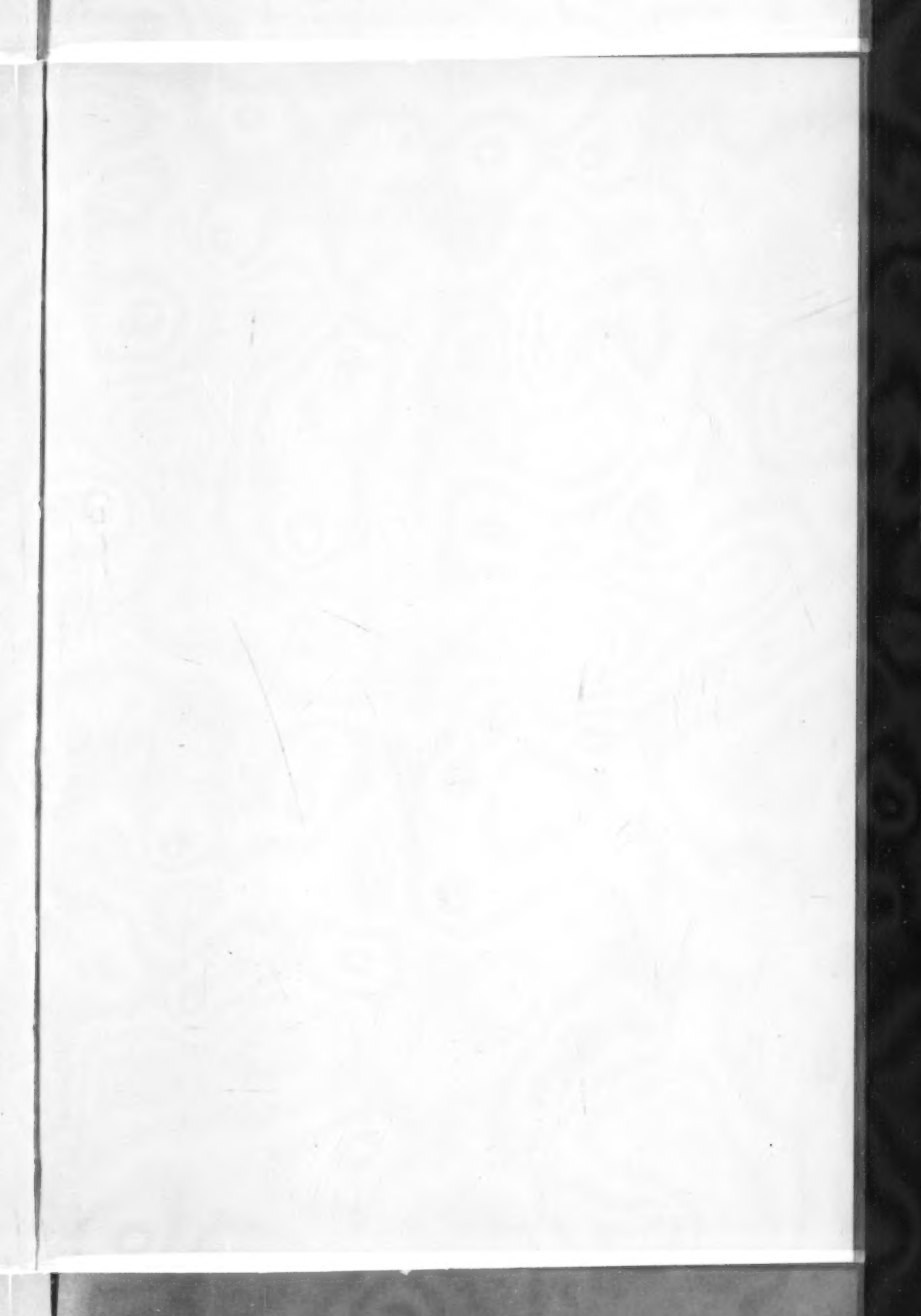
Approx. No. Psychol. Books in Library	Names of Psycho- logical Journals in Library	Chief pieces of Apparatus and Equipment	No. Rooms for Psychol.	
			Entire Use	Part Use
	(List on re- verse side.)			

Remarks:

Name.....

Official Position.....

\* A similar sheet, appropriately modified, was also sent for educational psychology.







## Psychological Review Publications

Original contributions and discussions intended for the *Psychological Review* should be addressed to

Professor Howard C. Warren, Editor *Psychological Review*,  
Princeton University, Princeton, N. J.

Original contributions and discussions intended for the *Journal of Experimental Psychology* should be addressed to

Professor Madison Bentley, Editor *Journal of Experimental Psychology*,  
University of Illinois, Urbana, Illinois.

Contributions intended for the *Psychological Monographs* should be addressed to

Dr. Raymond Dodge, Editor *Psychological Monographs*,  
Kent Hall, Yale University, New Haven, Conn.

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Clark University, Worcester, Mass.

All business communications should be addressed to

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## DIRECTORY OF American Psychological Periodicals

- American Journal of Psychology**—Ithaca, N. Y.; Cornell University. Subscription \$6.50. 624 pages ann. Ed. by M. F. Washburn, K. M. Dallenbach, Madison Bentley and E. G. Boring. Quarterly. General and experimental psychology. Founded 1887.
- The Pedagogical Seminary and Journal of Genetic Psychology**—Worcester, Mass.; Clark University. Subscription \$7.00. 700 pages ann. Ed. by Carl Murchison. Quarterly. Child behavior, differential and genetic psychology. Founded 1891.
- Psychological Review**—Princeton, N. J.; Psychological Review Company. Subscription \$5.00. 480 pages annually. Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.
- Psychological Bulletin**—Princeton, N. J.; Psychological Review Company. Subscription \$5.50. 720 pages annually. Psychological literature. Monthly. Founded 1904. Edited by Samuel W. Fernberger.
- Psychological Monographs**—Princeton, N. J.; Psychological Review Company. Subscription \$6.00 per vol. 500 pp. Founded 1895. Ed. by Shepherd I. Franz. Published without fixed dates, each issue one or more researches.
- Psychological Index**—Princeton, N. J.; Psychological Review Company. Subscription \$2.00. 200 pp. Founded 1895. Edited by W. S. Hunter. An annual bibliography of psychological literature.
- Journal of Philosophy**—New York; 515 W. 116th Street. Subscription \$4. 728 pages per volume. Founded 1904. Bi-weekly. Edited by F. J. E. Woodbridge, Wendell T. Bush and H. W. Schneider.
- Archives of Psychology**—Sub-Station 84, N. Y.; Archives of Psychology. Subscription \$5. 500 pp. per vol. Founded 1906. Ed. by R. S. Woodworth. Published without fixed dates, each number a single experimental study.
- Journal of Abnormal Psychology and Social Psychology**—Albany, N. Y. Subscription \$5. Boyd Printing Co. Ed. by Morton Prince. In cooperation with Henry T. Moore. Quarterly. 432 pages ann. Founded 1906. Abnormal and social.
- Psychological Clinic**—Philadelphia; Psychological Clinic Press. Subscription \$2.50. 288 pages. Ed. by Lightner Witmer. Founded 1907. Without fixed dates (9 numbers). Orthogenics, psychology, hygiene.
- Training School Bulletin**—Vineland, N. J.; The Training School. Subscription \$1. 160 pages ann. Ed. by E. R. Johnstone. Founded 1904. Monthly (10 numbers). Psychology and training of defectives.
- Comparative Psychology Monographs**—Baltimore; Williams & Wilkins Co. Subscription \$5. 500 pages per volume. Edited by W. S. Hunter. Published without fixed dates, each number a single research.
- Psychoanalytic Review**—Washington, D. C.; 3617 10th St., N. W. Subscription \$6. 500 pages annually. Psychoanalysis. Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.
- Journal of Experimental Psychology**—Princeton, N. J. Psychological Review Company. 480 pages annually. Experimental. Subscription \$5.00. Founded 1916. Bi-monthly. Ed. by Madison Bentley.
- Journal of Applied Psychology**—Bloomington, Ind.; Indiana University Press. Subscription \$4. 400 pages annually. Founded 1917. Quarterly. Edited by James P. Porter and William F. Book.
- Journal of Comparative Psychology**—Baltimore; Williams & Wilkins Company. Subscription \$5. 500 pages annually. Founded 1921. Bi-monthly. Edited by Knight Dunlap and Robert M. Yerkes.
- Genetic Psychology Monographs**—Worcester, Mass.; Clark University. Subscription \$7.00. 600 pp. ann. Ed. by Carl Murchison. Bi-monthly. Each number a complete research. Child behavior, differential and genetic psychology. Founded 1923.
- Psychological Abstracts**—Eno Hall, Princeton, N. J. Edited by W. S. Hunter. Subscription \$6.00. Monthly. 600 pages annually. Founded 1927.

